Readings in CHILD PSYCHOLOGY

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CHILD
PSYCHOLOGY

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edited by WAYNE DENNIS

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Preface

One characteristic that distinguishes child psychology from more recondite fields is that its subject matter frequently is treated by many self-appointed experts, authorities, and the like. He who can recall his own childhood is an expert; he who has reared children is an authority. Anyone who has read a book on child training considers himself to be a scholar. The person who accepts an invitation to speak about children also receives a revelation that permits him to discuss the subject with complete assurance. Thus, although reliable knowledge concerning children is scarce, there is no lack of confident opinion.

In this welter of wisdom, the only thing that marks off the psychologist is his method of obtaining knowledge. He does not gain it by insight, by intuition, or by accepting a system of doctrine. His information comes from tedious investigations that can be checked and rechecked by others.

Since ideas concerning child nature and child care are so apt to be affected by prejudice and dogma, it has seemed to me particularly important in choosing the selections for this book to include a very liberal representation of scientific studies of child behavior. These will, I hope, demonstrate how facts about children can be ascertained, and how one is to distinguish between fact and opinion.

In addition to factual investigations, theoretical discussions of several problems are included. However, the theories that are presented represent several different approaches. A definite attempt has been made to avoid indoctrinating the student in any one point of view; the book is neither behavioristic or psychoanalytic, neither Lewinian nor Rogerian, although some representation is given to the concepts of each of these schools.

One of the considerations that affected the choice of selections for this volume was that of avoiding duplications of materials already available to the student in other compilations. Thus, some well-known contributions to child psychology were omitted because they are included in Barker, Kounin, and Wright's *Child Behavior and Development,* in Kluckhohn and Murray's *Personality in Nature, Society, and Culture,* in Hartley and Newcomb's *Readings in Social Psychology* or in my own *Readings in General Psychology* or *Readings in the History of Psychology.* Nevertheless, an attempt was made to give adequate representation to every major subdivision of child psychology.

I wish to express my appreciation of the generous response of authors and publishers to requests for permission to use the selections reprinted in this volume. Specific acknowledgments of sources of original publications are given in connection with the respective selections.

Finally, I wish to indicate that the survey of the extensive literature of child psychology on which this book is based would have been impossible without the continuous assistance of my wife in this project and in related tasks over a period of many years.

WAYNE DENNIS

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READINGS IN
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I–BEHAVIOR OF THE FETUS AND NEONATE

One—The Development of Behavior in the Human Fetus

DAVENPORT HOOKER

THIS SELECTION CONSTITUTES PART OF A LECTURE DELIVERED AT THE UNIVERSITY OF MICHIGAN AND PUBLISHED BY THE UNIVERSITY OF MICHIGAN IN 1944 UNDER THE TITLE *The Origin of Behavior*. IT IS REPRINTED (PAGES 19–35) BY PERMISSION OF DR. HOOKER AND THE UNIVERSITY OF MICHIGAN. THE ILLUSTRATIONS ARE REPRINTED WITH DR. HOOKER'S PERMISSION FROM *A Preliminary Atlas of Early Human Fetal Activity*, WHICH WAS PRIVATELY PRINTED BY DR. HOOKER IN 1939.

The earliest human responses can be observed directly only on those relatively rare occasions when medical necessity requires the removal of the fetus. Over a period of many years Hooker has had the cooperation of the medical profession in a large metropolitan center in making such fetuses available for observation. Hooker's series of cases is by far the most numerous yet reported, and his studies are likely to be the authoritative ones in this field for many years. In addition to the extensiveness of his series, Hooker has had the further advantage, not shared by earlier investigators, of being able to record by means of the motion picture camera the fetal behavior that he has studied. These records make it possible for unusual opportunities for observation to be shared permanently by other scientists. They also enable any researcher to observe repeatedly in order to arrive at a careful analysis and evaluation.

The behavior of the fetuses of infrahuman animals has been studied very extensively. If the student should wish to compare the human observations presented in this chapter with the methods and results of related animal studies, he may find a comprehensive survey of fetal behavior by Leonard Car-

michael in his Manual of Child Psychology. *Baltimore: John Wiley and Sons, Inc., 1946*

Before giving you the data that I have gathered on the prenatal development of human behavior, it seems wise to discuss some of the conditions under which these human fetal observations were made and some of the methods used. These may give you an idea of some of the factors which might be considered as limiting the validity of observations on human fetuses. That such limitation exists is evident; that it constitutes seriously invalidating conditions may be doubted.

First, we may consider the question of age determination. For the time being all that is necessary is to establish an approximate sequence according to age of the fetuses observed. No absolute accuracy may be claimed for the menstrual ages used in this presentation. They may differ from the true menstrual ages by a week or more in either direction. We have very few data which are reliable in regard to the actual menstrual histories of the fetuses. Although many other tables to determine menstrual age on length exist, those compiled by Streeter have been used in this work because they seem to fit best the type of population found in Pittsburgh. In the main, it has been found that determination of menstrual age from the Streeter or almost any other tables causes the performance of the fetuses observed by us to fall into a definite sequence, with unexpectedly few exceptions. Age determination is important, but for the purpose of this discussion only relatively so, provided a sequence may be established.

Second comes the question of the nature of the stimulus. It soon became evident, in the course of the observations reported here, that it was necessary to refine the methods of stimulation if an attempt was to be made to secure responses to what was hoped would be tactile or touch stimulation. Thus, stimulators have been made from hairs roughly graded by what may be considered as the maximum pressure they could exert. Hairs just stiff enough to exert pressure balancing weights of 10, 25, 50, and 100 milligrams and 2, 5, and 10 grams were used. A small bead of Duco cement was placed on the tips of the hairs to prevent abrasion of the fetal skin.

Of course, no evidence whatever is available regarding the area of that part of the surface of the bead in contact with the surface of the fetus, which from a physicist's point of view is essential to determine pressure. However, the stimuli are applied in such a way that the pressure exerted by the hairs as they are used is much less than the maximum which they are capable of exerting.

CONDITIONS OF OBSERVATION

I wish now to note certain factors which introduce abnormal conditions into observations of this type. These factors are dependent upon the technics which must be employed. The majority of our material to the menstrual age of twenty-five weeks has been secured by hysterotomy. Such operations are performed only for carefully determined medical reasons and, of course, no member of our group has ever had any part in the decision.

When a fetus is removed from the uterus, the connection between the fetal and the maternal portions of the placenta is necessarily interrupted. In consequence, anoxemia or oxygen want begins at once and becomes progressively more severe. It has been impossible to overcome this by any means so far used. How damaging to the conclusions to be drawn this progressive oxygen want may be is a matter of dispute. We believe that the responses of even the youngest fetuses are true neuromotor responses in our series, an opinion based on a number of considerations.

Another abnormal factor is the release of the normal pressures exerted upon the fetus in the amniotic sac within the uterus. Furthermore, there is at least a temporary breakdown of environmental temperature control. Of course, a certain amount of physical insult is suffered by the immature fetus during operative procedures or during premature birth.

Many anesthetic drugs administered to the mother pass readily through the placenta and narcotize the fetus to varying degrees. In my experience, only novocaine, in its various forms, appears not to affect fetal movements when used either as a spinal or local anesthetic.

In examining a nonviable fetus our routine has been to transport the fetus as quickly as possible to the observation laboratory, place it in warmed saline or Tyrode's solution under the cameras and place the placenta in a separate jar under a moist atmosphere of oxygen. The time elapsed between beginning placental separation and the time of initial observation is in the neighborhood of one and a half to two minutes in most cases.

Spontaneously delivered premature infants (after twenty-five weeks of menstrual age) are resuscitated, if possible, placed in a heated "premature bed," supplied with oxygen and examined before being taken to the nursery.

We have used motion picture recording extensively in our studies. Motion pictures are objective records which can be examined repeatedly. A quick and intricate response, seen once in the fetus, is most difficult to analyze. When rechecked repeatedly on the films, its exact nature may be determined with a high degree of accuracy. For this reason, motion picture records are almost essential.

EARLIEST RESPONSE

The studies at Pittsburgh cover to date 98 individuals. These range from seven weeks of menstrual age to a postmature of forty-five weeks (forty weeks being the usual normal gestation period based on menstrual age). Before eight and one-half weeks, approximately, reactivity has been possibly observed in only one case, one of dichorial twins at seven and one-half weeks. Stimulation of the region about the mouth resulted in a contralateral flexion of the neck, seemingly without participation of the upper extremities. Unfortunately, this response is not shown in the very poor motion pictures we secured. However, Fitzgerald and Windle (2) describe a fetus of this age from which they elicited this same response by what they describe as "strong mechanical stimulation of the maxillary region." In our case, strong stimulation was unnecessary.

Shortly after eight weeks of menstrual age, as determined by the Streeter tables, the reflexogenous area was found to lie over the upper lip and wings of the nose supplied by the maxillary division of the fifth cranial nerve, the trigeminal. Almost immediately thereafter the reflexogenous area extends to the upper lip, chin, and for a short distance onto the neck, supplied by the mandibular division of the same nerve.

The characteristic response at this age is of one type only and may be elicted by 10 milligram or stiffer hairs. It is a lateral flexion of the trunk and neck, usually contralateral, but sometimes ipsilateral, accompanied by slight but definite rotation of the pelvic region, in the same direction as the trunk flexure. The trunk flexure is also accompanied by an extension of both arms (using the term as the equivalent of the brachia or upper arms). The elbows, which are normally about half flexed at this age, do not participate, nor do the hands at the wrist, though the hands describe an arc as a result of the arm extension at the shoulder. The return to normal resting posture by the arms precedes that of neck, trunk, and rump, which maintain the reaction posture for an appreciable period of time before they also return to resting posture.

The response is sterotyped, that is to say, it is patterned and mechanical in nature. These fetuses (and I use the term fetus for all, rather than to distinguish between embryos and fetuses) are fairly active over the relatively short period of time during which they remain responsive to stimulation. However, each stimulation in the reflexogenous area evokes a response which, within the limits of ordinary biological variation, is identical with every other one secured. No "spontaneous" movements have been observed at this

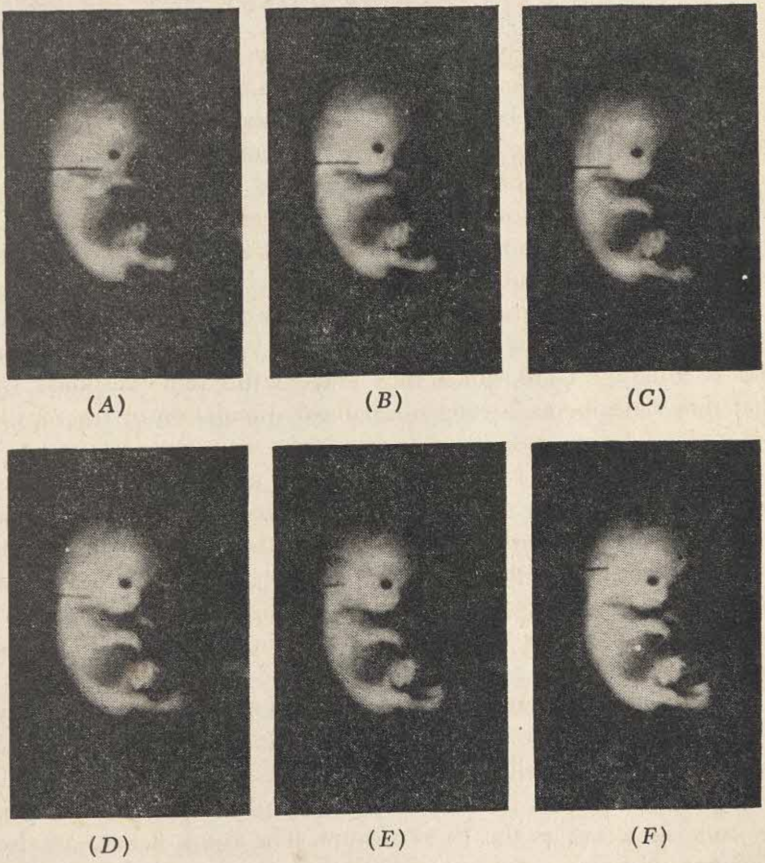

*Figure 1. Response to tactile stimulation of the face (eight and one-half weeks). Stimulation was applied by stroking the integument lateral to the nose and mouth (*A *and* B*) with a fine hair. The hair, as seen in the other figures, was not in contact with the fetus. The response consisted of the following elements:*

*1. Contraction of the long muscles of the neck and trunk on the side of the body opposite to that stimulated, causing a contralateral body flexion (*B *to* E*). A barely perceptible rotation of the rump also occurred.*

*2. A quick backward movement of both arms, with caudal excursion of the hands (as the forearms are at an angle with the arms), caused by contraction of the girdle muscles with the long muscles of the back (*B *and* C*).*

*3. Maintenance of the reaction posture of the trunk (*D, E, F*) and of the upper extremities.*

4. Return to normal fetal posture of the trunk and of the upper extremities (not shown).

age. From my viewpoint, spontaneous movements occur in response to unknown stimulation, which may originate either within or without the organism.

At nine and one-half weeks, there has been no extension of the reflexogenous area. The response is similar to that secured at eight and one-half weeks, but is more marked. The characteristic response is again a lateral, usually contralateral, flexion of neck and trunk, with rump rotation and bilateral arm extension, but both lateral flexion and rump rotation are much more marked than at eight and one-half weeks. However, the responses are patterned and mechanical in nature, hence to that extent stereotyped. The fetuses of this age are active in responding, and exhibit spontaneous movements identical in character with response activity but occurring in rhythmic contractions of the body, first to one side, then to the other.

Two fetuses of nine and one-half weeks' menstrual age exhibited movements believed to be vestibular in origin. These movements occurred following rolling of the fetus to adjust it under the cameras. The vestibular apparatus appears sufficiently well developed at this age to justify the belief that the semicircular canals are functional.

Responses, clearly proprioceptive in nature (that is, originating from nerve endings in the muscles, tendons, or joints) have been secured for the first time at this age by stretching the forearm muscles in one instance and the biceps muscle in another.

At ten and ten and one-half weeks we have insufficient data for proper conclusions, but I would like to note that at ten weeks the eyelids are completely developed, though fused (as in a kitten at birth), and that the metanephros, the definitive kidney, is well developed at this time. Guthmann and May (3), it might be noted in passing, demonstrated the presence of urea and uric acid in the amniotic fluid at this age, indicating urination by the fetus. It is often overlooked that kidneys, bowels, and lungs may begin to function before birth in spite of the supply and eliminative functions of the placenta.

At eleven weeks, the reflexogenous areas have spread over a considerable additional portion of the body surface. As heretofore, the maxillary and mandibular areas of supply are functional, but now the ophthalmic division of the trigeminal nerve, supplying the upper eyelids and forehead, exhibits capacity to function and the skin of the upper extremities, notably in the palms, also becomes reflexogenous. Only in the older fetuses, nearing eleven and one-half weeks, is the skin of the arms and forearms definitely reflexogenous. In the oldest classified at this age, a foot sole response was secured.

ELEVEN TO FOURTEEN WEEKS

At eleven weeks, extension of the trunk is beginning to be combined with, or substituted for, the lateral flexion hitherto present as the only response to stimulation in the facial reflexogenous area. All manner of combinations of extensions and lateral flexion may be present and the response to stimulation in the facial area may be segmental in character. In the segmental type of response, the upper part of the body is followed by the lower part in executing movements, as though a lag existed as the impulse passes caudally. Arm extension may or may not accompany the trunk extension. If lateral flexion is present, either as a component or as the sole response, arm extension is usually present. In general, the deeper oxygen want the greater is the tendency for the response to be largely or entirely a lateral flexion. This would seem to indicate that the older type of response tends to persist longer than the newer type as lack of oxygen progresses, though lateral flexion may be present at any time during the period when the fetus will respond to stimulation.

If the palm of the hand is stimulated with a relatively pliable hair at this age, a quick closure ensues. A study of the motion picture proves the finger closure to be only partial. Halverson (4) has divided prehension into two phases, finger closure and grasp. These two phrases are sequential in development. No grasping is present at this stage, indeed, as I have indicated, the initial finger closure exhibited at eleven weeks is incomplete. In this finger flexion, the thumb may or may not be involved. In earlier observations at this age, thumb movement was absent, but it has been seen to occur in more recently observed specimens of this age.

In the oldest fetus assigned to this age group, plantar stimulation caused plantar flexion of the toes. (Plantar flexion of the toes being their flexion toward the sole of the foot, dorsiflexion being their extension toward the dorsum of the foot.) Furthermore, forearm pronation followed stimulation over the arm, and stimulation of the eyelids caused a contraction of their muscles.

All of the responses at this age are mechanical in character and, hence, stereotyped. However, their stereotypy is modified by the fact that a greater or less component of trunk and neck extension may be present.

In the eleven and one-half weeks menstrual age group, the reflexogenous areas have extended to include the entire face, the ventral neck, the upper chest, and the upper and lower extremities. Here, stimulation in the nose-mouth area evokes neck and trunk extension as a major component, though lateral flexion still is present. The arms rotate medially, so that the hands approach one another in front of the chest, as an accompaniment of trunk movements. In addition, movements involving thigh and leg extension, head rotation, arm abduction and rotation, or elbow and wrist flexion may occur as isolated responses or components of other more general bodily reactions. Finger closure is as yet incomplete. Stimulation of the sole of

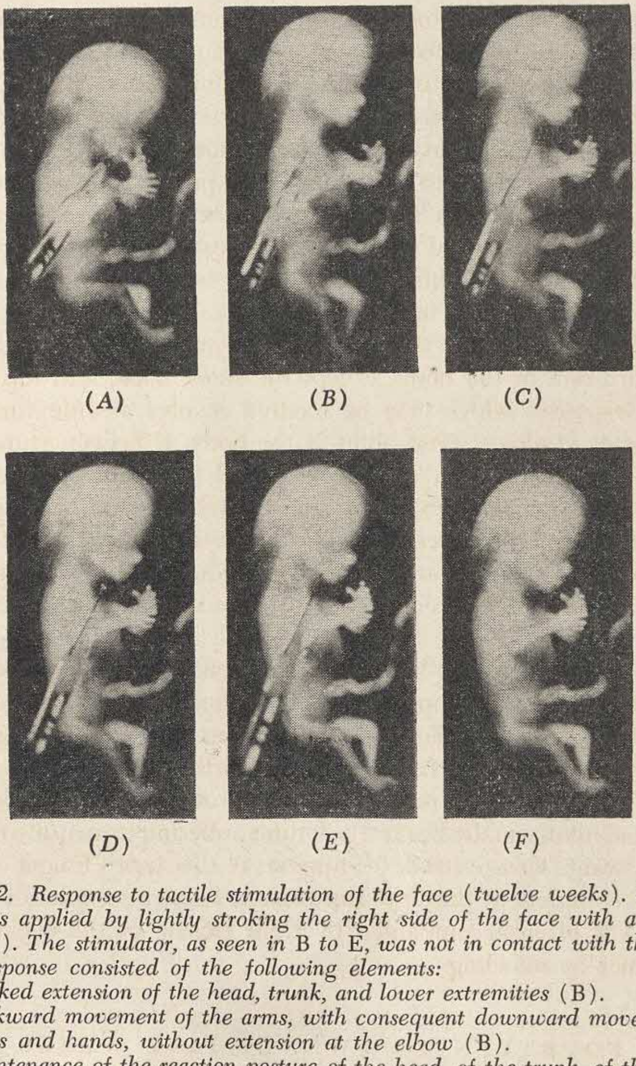

Figure 2. Response to tactile stimulation of the face (twelve weeks). Stimulation was applied by lightly stroking the right side of the face with a 2 gram hair (A). *The stimulator, as seen in* B *to* E, *was not in contact with the fetus. The response consisted of the following elements:*

1. *Marked extension of the head, trunk, and lower extremities* (B).
2. *Backward movement of the arms, with consequent downward movement of forearms and hands, without extension at the elbow* (B).
3. *Maintenance of the reaction posture of the head, of the trunk, of the upper extremities, and of the lower extremities* (C *and* D).
4. *Return to normal posture of the head, of the trunk, of the upper extremities, and of the lower extremities* (D *to* F). *It will be noted that the terminal posture* (F) *does not show complete return to the initial posture.*

the foot may cause either plantar flexion or dorsiflexion of the toes, both types being present in one fetus at different stimulations and sometimes alternating with one another. There is no toe-fanning at this age. Toe responses are ordinarily accompanied by ankle, knee, and hip flexion. This response is still undeveloped, but presages the more complete plantar reflex as a stage in its progressive perfection.

At twelve weeks, there has been little further extension of the reflexogenous areas. Trunk and neck extension have become the characteristic response to stimulation in the nose-mouth area, with or without one or more of a wide variety of movements of the extremities. These movements of the extremities may occur as a result of flexion or extension at almost every joint. Digital closure is still only partial, but the foot-sole response is progressing toward completion, the toes plantar flexing or dorsiflexing with some fanning. Contraction of the eyelid muscle may be readily secured and, in one case where the highlights were suitable, movement of the eyeball behind the eyelids was indicated.

By thirteen weeks, the reflexogenous areas include practically the entire skin of the body, except the sides, back, and top of the head. Responses which may be secured involve a wide variety of movements at almost every joint of the body. However, stimulation in the nose-mouth area still evoked neck and trunk movements, chiefly extension. Thumb opposition, though only partial, has been observed when the other digits flexed. Swallowing, suspected but not proved in one case at twelve and one-half weeks, definitely occurred at thirteen weeks and thereafter on stimulation between the lips.

At thirteen and one-half weeks, the reflexogenous area includes the sides, but not the top or back of the head, a distribution found until birth. Very complicated combinations of specific reflexes are characteristic of fetuses from this age to birth. The more generalized type of neck and trunk responses hitherto observed are, at this age, only incidental, usually limited to fetuses suffering from quite marked oxygen want. Crossed reflexes appear at this time. Finger closure may be complete at this age. Lip closure follows stimulation of the lips and is often accompanied by tight closing of the eyelids and sometimes by scowling.

FOURTEEN TO TWENTY-FIVE WEEKS

Fourteen weeks is an important age landmark in the development of the fetus. The fetus is no longer marionette-like or mechanical in the character of its movements, which are now graceful and fluid, as they are in the newborn. A wide repertoire of specific reflexes may be elicited throughout the entire body and these activities are exhibited spontaneously. Stimulation in the mouth-nose area no longer causes a general trunk response, but evokes facial responses and a rotation of the head away from the stimulus.

Stimulation near the sides of the nose produces an elevation of the upper lip and wings of the nose resembling a sneer. Scowling and an avoiding movement of the head usually accompany the lip response. Stimulation of the tongue evokes its movement and, if repeated, swallowing.

At fourteen weeks, the fetus exhibits, in imperfect form at least, most of the reflexes of the neonate, except respiration, voice, grasp, suctorial response, tendon reflexes, and special sense responses. No tests of the special senses were made in these observations. From this age to birth, the character of the fetal activities remains much the same, with the emphasis on the specific reflexes. In consequence, I shall note only those activities which appear *de novo* or which reach important stages in their development.

By fifteen weeks, the abdominal reflex becomes definite in its appearance, though it is possible that it may be present earlier. If, at this age, a hair is drawn across the palm, the fingers not only close, but remain closed, particularly if the hair is left within the closed fingers. This seems to be the initial appearance of the grasping reflex though there is no effective clutching of the object within the palm.

By about sixteen weeks of menstrual age, an interesting change in the activity of the fetus has appeared. The younger fetuses observed have been active in their response to stimulation, but they now show a marked decrease in activity, becoming almost sluggish, although a few individuals have been observed which were still fairly active in their responses. Furthermore, where it may be accomplished, resuscitation causes disappearance of this inactivity and increase in threshhold, otherwise rather characteristic of the period from about sixteen weeks to birth.

At seventeen weeks, stimulation of the lower lip causes lip closure, as heretofore, but stimulation of the upper lip evokes its marked protrusion. This is believed to be the initial phase of the developing suctorial response.

At eighteen weeks, stimulation of the chest wall has evoked thoracic contractions, interpreted as the first evidence (so far as these observations are concerned) of the ability of the chest wall to function in a respiratory manner.

At twenty weeks, both lips protrude when stimulated. By twenty-one weeks, the separation of the eyelids from one another, begun at about eighteen weeks, is usually completed, but the fetus rarely opens the eyes, either spontaneously or in response to stimulation, until later.

By twenty-two weeks, some fetuses are capable of briefly sustained respiration. That this is true inspiration and expiration of air is proved by the fact that, while it is respiring, the fetus is capable of making a thin crying noise on expiration. At this age, also, the lips are not only protruded but pursed, when a stimulating object touches both at the same time. There is, however, nothing which suggests sucking. Furthermore, the ability of the fetus to grasp an object has reached a point in the maturation of this reflex at which it is somewhat effective. It is sufficiently strong to enable the observer to move the upper extremity by pulling a grasped rod in the direction of its long axis. It is interesting to note that the thumb plays no part in the grasping reflex.

At twenty-three weeks of menstrual age, a resuscitated fetus is capable of about a half-hour of self-sustained respiration and, at twenty-four weeks such respiration may continue for over three hours.

TWENTY-FIVE WEEKS TO NORMAL BIRTH

Twenty-five weeks of menstrual age marks another important landmark in the maturation of fetal reflex behavior. Self-sustained respiration, after resuscitation, may continue for more than twenty-four hours. Though I have not found any premature younger than twenty-seven weeks to be viable, even with the utmost care, cases have been reported of viable twenty-five weeks premature infants. Following resuscitation, certain tendon reflexes (knee-jerk, ankle clonus, and so on) can be elicited. The individual must, in my experience, have been respiring for nearly half an hour before any tendon reflexes can be secured. They have been sought in younger fetuses, but not found until this age, at which sustained respiration over a relatively long period is possible.

At twenty-five weeks, the eyes may be opened and closed spontaneously.

One twenty-five weeks premature, within a half-hour of birth, was gently lifted from the "premature bed" to change, for photographic purposes, the cloth on which it was lying. When this had been done, it was lowered suddenly into the bed. The response consisted of a rotary gyration of upper and lower extremities, fully abducted from the trunk, followed by the drawing of these extremities over the trunk. This is generally termed a Moro, or "startle" reflex. It is undoubtedly vestibular in origin.

At twenty-seven weeks of menstrual age, one fetus proved viable. This one exhibited a strong grasping reflex, sufficiently strong indeed to support almost its entire weight by one hand. During this grasp, the thumb was actively flexed but the weight was supported by the fingers, not by the thumb. It is not until after birth that the thumb plays an active role in grasping.

At twenty-nine weeks, an audible suctorial response was exhibited. At thirty-two weeks, a definite cremasteric reflex was secured several times from the male premature infant. It is believed that this reflex may be present earlier, but unmistakable proof in the motion pictures is lacking. At thirty-three weeks, when the lower lip alone was stimulated, the mouth was opened and the tip of the tongue "searched" the lower lip, following the stimulus.

No additional data have been recorded in older ages to the time of birth of the prematures or of the postmature observed. Study of the motion picture records of ages older than twenty-five weeks is not yet complete and many additional observations must be made.

NATURE OF FETAL DEVELOPMENT

The account given of the findings during the early phases of the development of overt behavior in man in the Pittsburgh studies should clearly set forth where I stand concerning the nature of this development. These earliest forms of response exhibited by man certainly constitute a total pattern, in Coghill's sense (1). This total pattern expands to include additional elements of the neuromuscular apparatus as it develops. Following this, an individuation of specific reflexes or partial patterns occurs.

In these respects, the development of early overt behavior in man follows the basic principles discovered for *Amblystoma* by Coghill. However, the development of early overt behavior in man is not the same as that in *Amblystoma*, nor is it the same as that in the cat, the rat, the rabbit or the sheep. Each animal form exhibits a sequence in the development of its early overt behavior which is entirely its own. In man, compared to some of the other mammals, this early sequence appears to be curiously telescoped in certain parts. However, the development of human fetal activity, as seen in our studies, is not only consistent with Coghill's principles, but no other interpretation seems justified.

In the course of the later development of human overt behavior, it might be suggested that the specific reflexes, once individualized as partial patterns from the total pattern, may be recombined into an almost infinite variety of action patterns which form the background of adult human behavior. This is probably true of all mammals, but I can speak with some degree of assurance only concerning man.

SUMMARY

In closing, permit me to review, once again, the steps in the development of human fetal overt behavior, as demonstrated by the work at Pittsburgh:

1. It begins at about seven and one-half to eight weeks of menstrual age as a lateral, almost uniformly contralateral, contraction of the upper trunk and neck musculature. Possibly the earliest form of this response does not include movement of the limbs, but these, particularly the upper extremities, are very early coordinated with the trunk.
2. The initial response quickly spreads caudally to include the lower trunk and hips in the contralateral body flexion.
3. Beginning at ten and one-half to eleven weeks, specific reflexes of localized extent appear and the contralateral trunk flexure gives way to a trunk extension.
4. By thirteen to forteen weeks, almost all the important movements of the fetus have the form of specific reflexes which have already begun to recombine into an amazing variety of complex activities.
5. From fourteen weeks to birth, some new components of activity appear. Among these are: the suctorial response, respiration and the use of the larynx, the grasping reflex, and so on. Otherwise, the development of the fetus during these six to seven lunar months is one of maturation of already existing reflexes and of their essential neuromuscular substrate.
6. These fetal activities are basic and integral parts of postnatal human behavior.
7. This sequence of appearance of reflex activity is entirely consistent with Coghill's principles of the development of behavior.

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Two—Fetal Reactivity to Sound

JACK BERNARD &
LESTER W. SONTAG

REPRINTED FROM AN ARTICLE ENTITLED "FETAL REACTIVITY TO TONAL STIMULATION: A PRELIMINARY REPORT." *Journal of Genetic Psychology, 70 (1947)*, 205-210, BY PERMISSION OF THE SENIOR AUTHOR AND THE JOURNAL PRESS.

Sontag and his collaborators at the Fels Research Institute have conducted a series of studies on the behavior of the fetus in the later months of pregnancy. They have made use of several types of indirect indicators of fetal activity: The mother's report of fetal responses has been used, recording tambors have been applied to the mother's abdomen, and the fetal heart sounds have been picked up by microphone, amplified and recorded. The latter method is employed in the following study. As the authors indicate, much knowledge of the causes and consequences of fetal movements in late pregnancy remains to be gained through future research.

In 1925 Peiper (1), using a "loud and shrill" automobile horn as the stimulus and fetal movement as the index of reactivity, obtained some fetal responses, but cautiously concluded that the results of his experiment were ambiguous. More recently Sontag and Wallace (2, 3) and Sontag and Richards (4) have produced evidence that the fetus in utero is responsive to a 120-cycle vibratory stimulation applied to the maternal abdomen, responding by cardio-acceleration and by muscular activity in a way which is suggestive of a fetal startle pattern.

In this connection, it is pertinent to consider the fact that infants born prematurely are responsive to sound stimulation. It may be assumed, therefore, that the seven- or eight-month fetus possesses the necessary equipment for hearing. But, within the uterine environment, does the fetus hear?

The possibility that the human fetus in utero is capable of receiving and responding to stimulation which outside of the mother's abdomen is perceived as sound, opens an intriguing field of inquiry. When the vibratory stimulus used is evaluated as sound stimulation and calibrated for its sound qualities, such as tonal range and intensity, do we detect a differential response in the fetus in utero? Are sounds in the everyday external environment, such as music, the dropping of heavy objects, the sound of automobile horns, and so on, perceived by the fetus? How does the fetus' sensitivity to vibratory stimulation change with age? Another crucial question—and one which previous studies involving vibratory stimulation have failed to settle—is this: To be effective in arousing fetal response, must vibratory stimulation be applied directly to the mother's abdomen, that is, contiguous to the tissue of the mother's body? Or will vibration transmitted through air space be further transmitted through the mother's body to the fetus?

In an attempt to answer some of these questions, the following procedures were initiated.

PROCEDURE

Our experimental procedure involved the weekly presentation of tonal stimulation during the last two and a half months of pregnancy. The tones, relatively pure in character, were generated by a Jackson Audio Frequency Oscillator, amplified by a Setchell-Carlson Amplifier Model 13-B, and brought to the maternal abdomen through a Utah G8P speaker resting on a rubber baffle centered over the head region of the fetus. Thus any vibratory stimulus reaching the fetus would have to pass through an air space and could not be interpreted as being conducted through physical contact with the mother's body.

Fetal cardiac change was used as an index of fetal reactivity. Fetal heart sounds were picked up by means of a Brush microphone strapped to the maternal abdomen at the point where the sound was loudest, suitably amplified, and recorded by means of an ink-writer on a moving tape. A switch broke the microphone circuit as it made the speaker circuit, thus avoiding damage to the recording equipment.

During the weekly experimental sessions, each of which involved the presentation of approximately 10 tonal stimulations, the subjects rested in bed in a relatively soundproof room which the experimenter entered only for the purpose of adjusting the microphone. The frequencies used ranged from 20 to 12,000 dv/sec, and were varied systematically. Each stimulation lasted five seconds and was preceded by a warning about three seconds before. In the earliest work this warning was verbal; later a small signal lamp was substituted. In the present analysis, we shall consider only frequencies of 6,000 dv/sec and lower, as intensity fell off rapidly above that point with our present apparatus.

RESULTS

Ten-beat intervals were marked off on the records in both directions from the stimulation, measured as to length, and converted by means of a table to beats per minute. This analysis is therefore in terms of beats per minute of 10-beat samples. The 10-beat periods were continuous.

Figure 1 presents three individual curves for Subjects *F*, *H*, and *I*, and a composite curve for 73 stimulations conducted on these three subjects plus a few observations on a fourth subject, Subject *J*, who had too few stimulation sessions to permit treating her individual data statistically. It will be seen in each of these curves that a high fetal cardiac rate obtains shortly after stimulation. The critical ratio of the difference between the means of the five consecutive 10-beat prestimulation intervals and the three consecutive poststimulation intervals (when the effect of the stimulation was at its height) was highly significant in the case of each subject.

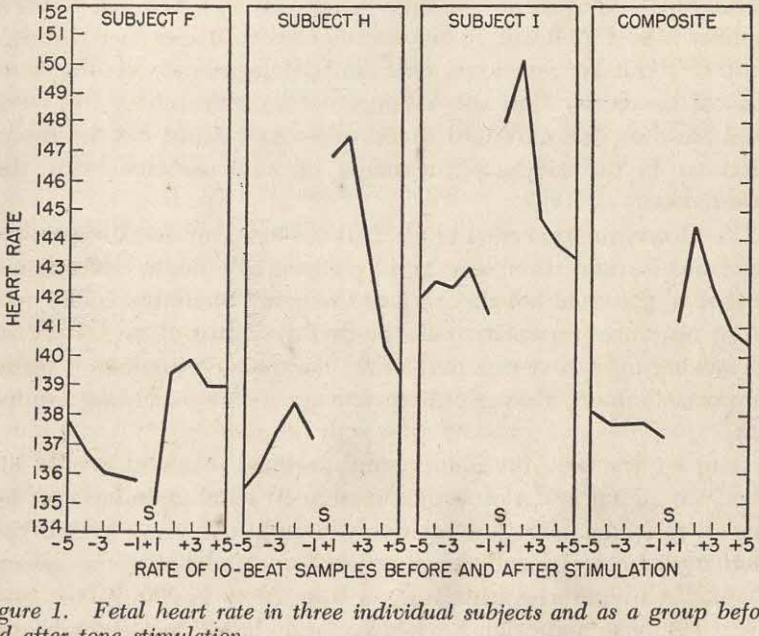

Figure 1. Fetal heart rate in three individual subjects and as a group before and after tone stimulation.

DISCUSSION

Four explanations of the phenomenon of cardio-acceleration visible in each of the curves appear plausible. We shall discuss each of these in turn.

1. Could the observed cardio-acceleratory response of the fetus be due to maternal stimulation with resultant humoral changes in the mother, which are then passed on through the placenta to the fetus to serve as a secondary stimulus? The four graphs in Figure 1 show that the cardio-acceleratory effect of the stimulation reaches maximum in the second 10-beat period following stimulation, or about 11 seconds after the onset of stimulation. The rapidity of this response eliminates the possibility of its being a maternal stimulus transmitted secondarily to the fetus through passage of newly liberated blood components into the fetal circulation. Any endocrine or other products released into the mother's blood stream as a response to stimulation could not possibly be circulated to the placenta and through the fetal circulation in so little time.

2. If the mother knew far enough in advance just when the stimulation was coming, is it conceivable that her emotional response might occur early enough to permit humoral changes to reach the fetal circulation? To test this possibility, we used suggestion of stimulation during 15 of the experimental sessions. On these occasions warning of impending stimulation was given, but no tone sounded. There was no significant change in fetal heart rate after such "suggestions." This fact does not rule out the possibility of a *delayed* fetal cardio-acceleration due to the process cited above, but it does eliminate the possibility that the cardio-acceleratory effect as demonstrated in this study is the result of humoral changes in the maternal organism.

3. Could the observed fetal cardio-acceleration be the result of mechanical stimulation caused by tensing abdominal muscles as part of a maternal startle response to the stimulation? No overt startle responses on the part of the mothers were noted during the study, and no mother reported being startled by the sound. Ample time was allowed to permit relaxation before each stimulation, and warning of impending stimulation kept the sound from being unexpected. In addition, the baffle concentrated the sound on the maternal abdomen and reduced the intensity reaching the maternal ear. These facts make it appear unlikely that the fetal response stems from *any* maternal reaction, either physical or chemical.

4. Finally, is the cardio-acceleratory response of the fetus a direct response to a stimulus which, evaluated externally as sound,

is transmitted to the fetus via the maternal tissues and/or fluids? In the light of the above discussion, this appears to be the logical explanation of the observed results.

The cardio-acceleratory effect shown in the curves is distributed over the full range of frequencies used, and is not the property of any narrow band. The problem of mapping the limits of fetal tonal sensitivity is therefore opened for future investigation.

By referring to Figure 1 it will be seen that the cardio-deceleration precedes cardio-acceleration for Subject *F*. This is in contrast to the direct acceleration shown in the other curves and is worth comment. Sontag and Richards (4, p. 49) have interpreted fetal cardio-acceleration following vibratory stimulation as an aspect of a fetal startle response. It is well known that the cardiac response in the human adult startle pattern involves a momentary deceleration followed by acceleration. It is possible that the same pattern obtains in the fetus, the deceleration in Subjects *H* and *I* taking place during the five-second stimulation period and thus not visible in our records, while for some reason not now apparent the deceleration in Subject *F* lasted beyond the stimulation period. This is a point which requires further investigation.

CONCLUSIONS

The experimental data presented here indicate that the human fetus in utero is capable of perceiving a wide range of tones. This tone sensitivity is such that tones produced by an amplifier loudspeaker arrangement and transmitted through the air to the mother's abdomen elicit a fetal response. Whether such tonal perception is accomplished by means of the fetal auditory apparatus or represents a vibratory perception sense of other parts of the body, has not been established. It may or may not be proper to speak of "fetal hearing." Response to tonal stimulation is expressed in sharp body movements and cardiac acceleration.

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Three—The Distribution of Activity Between Two Nursing Periods

ORVIS C. IRWIN

ABRIDGED FROM AN ARTICLE ENTITLED "THE DISTRIBUTION OF THE AMOUNT OF MOTILITY IN YOUNG INFANTS BETWEEN TWO NURSING PERIODS," *Journal of Comparative Psychology* 14 (1932), 415-428, BY PERMISSION OF THE AUTHOR AND THE WILLIAMS AND WILKINS COMPANY. DR. IRWIN KINDLY SUPPLIED THE PHOTOGRAPH THAT ILLUSTRATES THE TYPE OF EXPERIMENTAL CABINET USED.

The newborn infant of normal term may be studied by many methods, and the investigations of his senses and of his responses have been numerous. A full review of studies of the neonate has been presented by K. C. Pratt in Leonard Carmichael's MANUAL OF CHILD PSYCHOLOGY.

Several studies have been concerned with measuring the general activity of the young infant and with determining the effects of stimulating conditions and of physiological state upon the infant's activity. To accomplish these measurements, the infant is placed upon a platform that is sensitive to slight movements. The minute movements of the platform, which are caused by the infant's movements, are then recorded by pens writing upon a moving tape. This apparatus is called a "stabilimeter." In order to control the conditions surrounding the infant, the stabilimeter is enclosed in an experimental cabinet in which temperature, humidity, sound, light, and so on, can be regulated by the experimenter.

Irwin and his collaborators, using the apparatus described above, have contributed basic information concerning the activity of the child in the hospital period. The study that follows is concerned with the activity of the infant when no special ex-

ternal stimuli are applied. The infant is placed in the recording cabinet following one nursing period and remains there until the hour for the next feeding. The stimulus changes that occur during this time are, of course, primarily internal. The resulting activity is due primarily to the nutritional needs of the child.

In a previous study of four infants (1) the author reported that young infants are more active just before than after nursing. In the present article an analysis is made of the distribution of motility in newborn infants between two consecutive nursings.

SUBJECTS

The motility of 73 full-term infants was measured. With the exception of one infant thirty-two days old, the age range was from birth to sixteen days. Four of these infants were measured twice; the results, however, were considered as if they were those of different cases. The infants were born in the obstetrical department of the State University of Iowa General Hospital. They were fed at 2:00 and 6:00 P.M. The infants were placed on a stabilimeter at

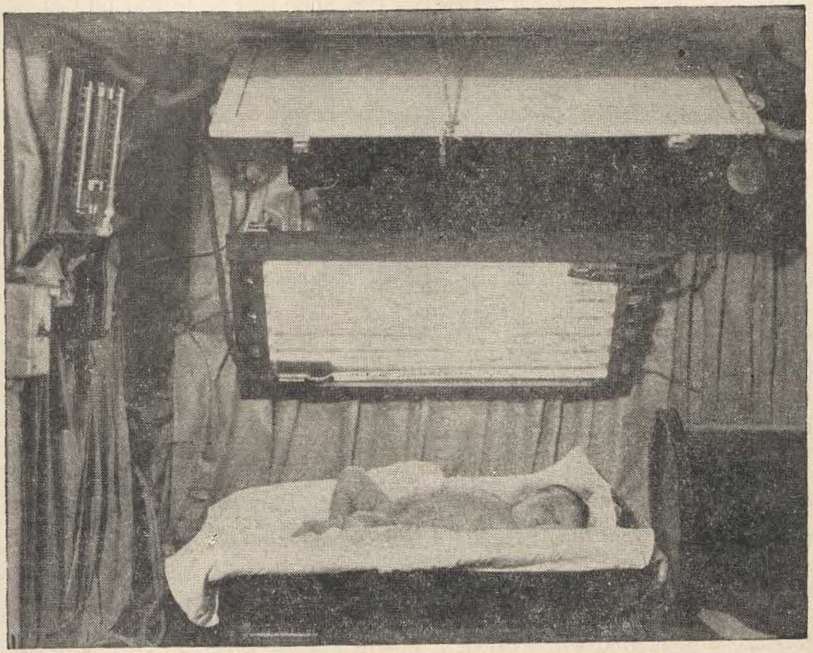

Figure 1. A stabilimeter cabinet used by Irwin.

2:30 P.M. and removed at 5:45 P.M., their motility being recorded continuously.

DISTRIBUTION OF MOTILITY

The mean oscillations per minute for each of the thirteen 15-minute periods for all infants are given in Figure 2, which also shows graphically how motility is distributed between the two consecutive feedings. According to Figure 2, there is a fairly constant increase in motility from the first 15 minutes of the experimental period when motility amounts to 17.0 oscillations per minute to the last 15 minutes when it amounts to 49.1 oscillations per minute. Thus the increase in motility toward the end of the experimental period is not quite three times the amount at the beginning. This finding verifies the observation made previously by the author that motility in infants is greater just before nursing than immediately after. However, as will be pointed out, this principle which is based on mean values, does not hold true in 100 per cent of the cases.

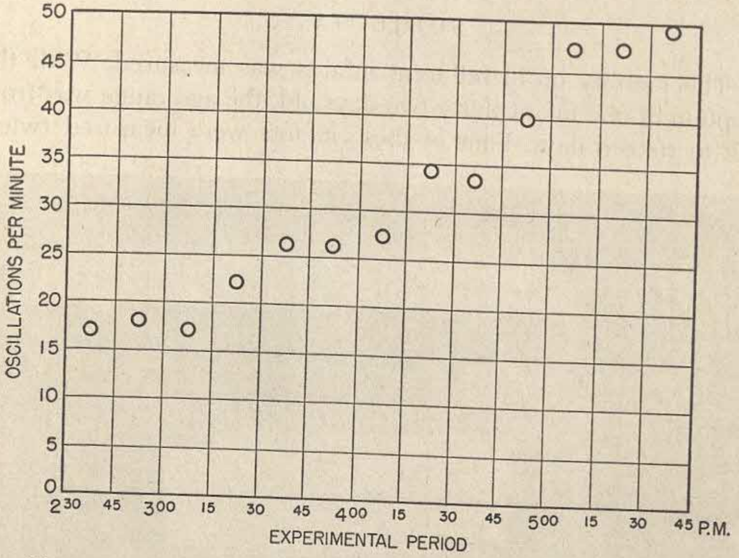

Figure 2. Motility of 73 infants between two consecutive nursings.

In order to determine whether the increase in motility as revealed by Figure 2 is a true increment, the standard errors of the differences between the first, middle, and last 15-minute periods were determined. It was found that the difference of the means of the first and last 15-minute periods was more than four times greater than the standard error of the difference. Hence the increment is a true one. The differences between the first and middle, and the middle and last 15-minute periods are not as great as in the case of the first and last periods, but they are still indicative of a significant tendency to differ. It would seem, then, that the increment of about three times the initial motility for the group as a whole is statistically significant on the basis of 15-minute samplings.

The distribution of motility between the two consecutive nursings as indicated by Figure 2 represents mean values for the whole group of 73 infants. These means hide an important trend in the data. Not all of the individual subjects show an increase in motility at the end of the experimental period. This is shown by comparing the motility of each infant for the first hour with that of the last hour. It is, then, found that 49 infants or 67 per cent of the entire group show an increment, while 24 infants or 33 per cent show a decrement in the amount of motility.

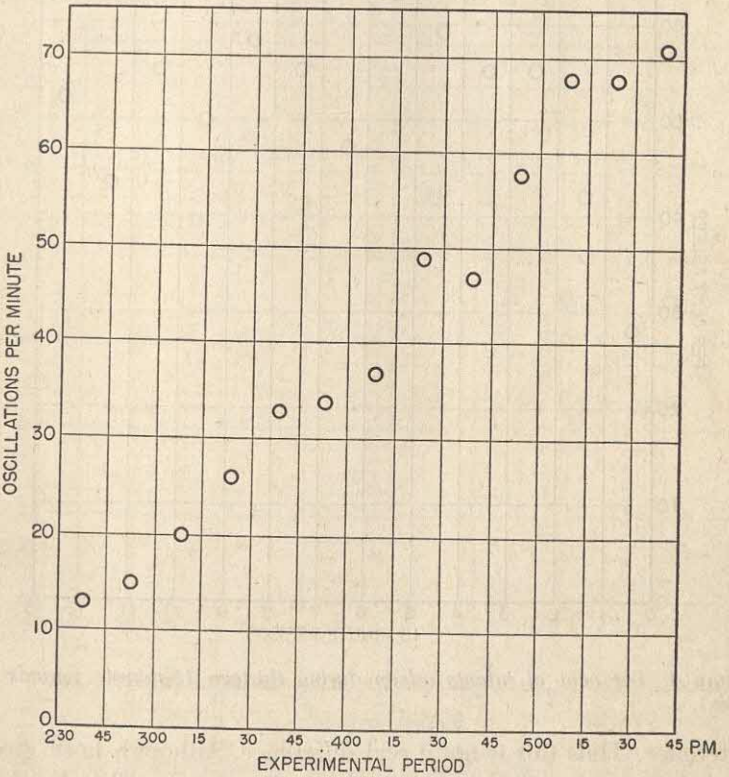

Figure 3. Motility of 49 infants showing increment.

However, the increment of the group of 49 infants showing increased motility is 3.5 times greater than the decrement made by the 24 cases. The mean oscillations per minute during the first hour for the group of 49 cases are 16.2 and for the last hour, 66.4. The difference of these means is about 7.5 times the standard error of the difference. It is, therefore, certain that this increment is significant. Figure 3 gives the motility curve of the 49 cases showing increments in motility.

The amount of motility of both groups during the first hour of the experimental period is about the same. The difference, five oscillations per minute, is only 0.27 times the standard error of the difference. Thus this is not a real difference. Although both groups start the experimental period at about the same motility level, the difference in the amount of their motility at the end of the experimental period is a significant one. The value of this difference for the last hour is 59.5 oscillations per minute, which is 8.8 times the standard error of the difference. Thus the negative effect of some of the infants is not sufficient to offset the general tendency of the whole group.

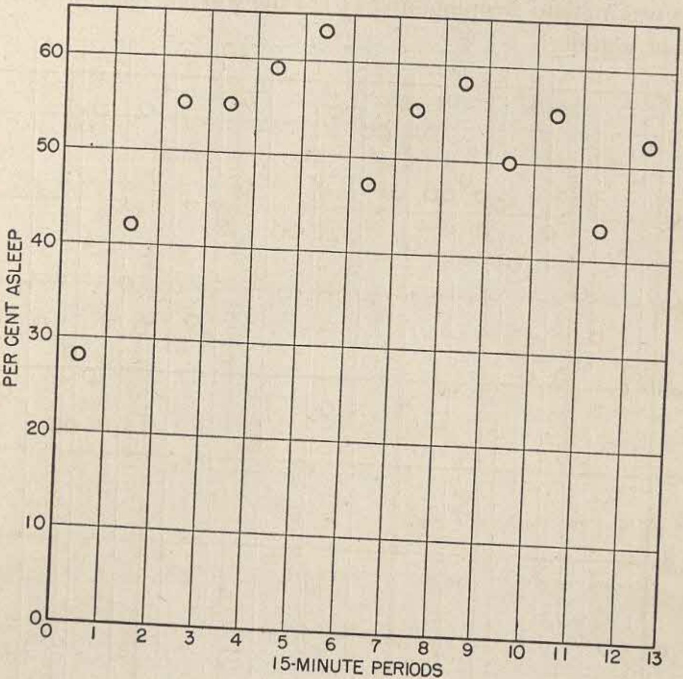

Figure 4. Per cent of infants asleep during thirteen 15-minute periods (31 cases).

It follows from these considerations that the increase at the end of the experimental period is statistically significant for the group as a whole, but that this increase is a resultant of a large increment made by two-thirds of the entire group and a lesser decrement by one third of the group.

PER CENT OF INFANTS ASLEEP AND AWAKE

There are certain problems which our data can answer. These are: (*a*) What per cent of the infants is asleep during the experimental period, (*b*) how does the amount of motility distribute itself during waking and sleeping, and (*c*) is the waking state itself a condition of activity?

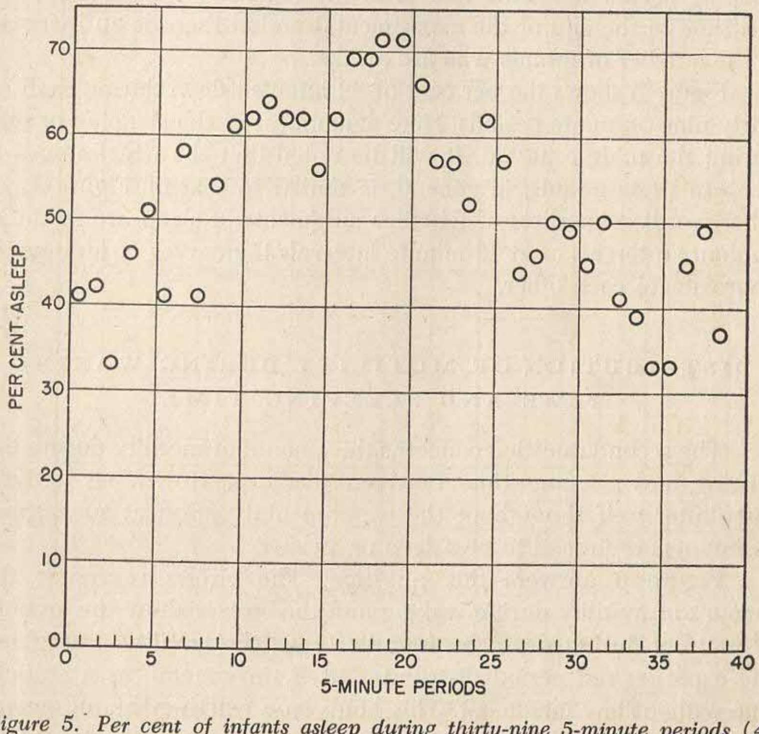

Figure 5. Per cent of infants asleep during thirty-nine 5-minute periods (42 cases).

The answers to each of these questions are given by two groups of data and their similarity affords a test of their reliability. With one group of 31 infants judgments of sleep were made at 15-minute intervals. With 42 subjects judgments were made of 5-minute intervals.

The first of the three questions concerns the per cent of infants awake and asleep throughout the course of the experimental period.

Figure 4 shows the per cent of 31 infants asleep during each of the thirteen 15-minute periods. During the first 15-minute period only 28 per cent of the subjects slept. Half or about 50 per cent of the infants slept during the last period. The largest number, 63 per cent, is asleep during the sixth 15-minute period. Thus the largest per cent of waking occurs at the beginning of the experimental period. This may be explained by the fact that at this time infants undergo handling. After being fed, they are carried from the mother to the nursery where they receive attention by the nurses. They are then brought to the experimental room, undressed, and placed on the stabilimeter in the experimental cabinet. The largest per cent of sleeping occurs about the middle of the experimental period. From this time to the end of the experimental period there is an increase in the number of infants who are awake.

Figure 5 shows the per cent of 42 infants asleep during each of thirty-nine 5-minute periods. Here again most of the sleeping occurs during the middle period. It will be noted that the distribution of sleep in these infants, in general, is similar to that of Figure 4. In other words, regardless of whether judgments of sleep are made at 5-minute intervals or at 15-minute intervals the curves of judgments approximate each other.

DISTRIBUTION OF MOTILITY DURING WAKING TIME AND SLEEPING TIME

The second question concerns the amount of motility during the waking and sleeping time between feedings. How does activity distribute itself throughout the experimental period at such times as infants are judged to be asleep or awake?

Figure 6 answers this question. The circles represent the amount of motility during waking and the crosses show the amount of motility during sleep. In the latter case, the motility throughout the experimental period distributes itself, in general, as a straight line without any increment. This holds true whether the judgments of sleep were made at 5- or at 15-minute intervals. On the other hand, motility during waking, beginning at the first part of the experimental period at a level about equal to motility during sleep, increases markedly throughout the period until it ends at a level about 10 times greater than the value for sleep. Moreover, this result is consistent for both intervals of observations.

These findings throw light on a problem which has been the source of much difficulty in experimenting with infants and which perhaps explains the variation in results of different experimenters.

The writer has suggested (1) that the effect of internal stimulation, such as hunger, results in activity so great as to interfere with the experimental use of external stimuli, such as sound, light, and so forth. It is important for the experimenter to know when the effect of internal factors is inoperative or at a minimum. In general, if inactivity in the overt behavior of the subject is a desideratum, these data show that the period following feeding is the best time to experiment on infants; if work on sleep is contemplated, the most suitable time for observation is the middle period.

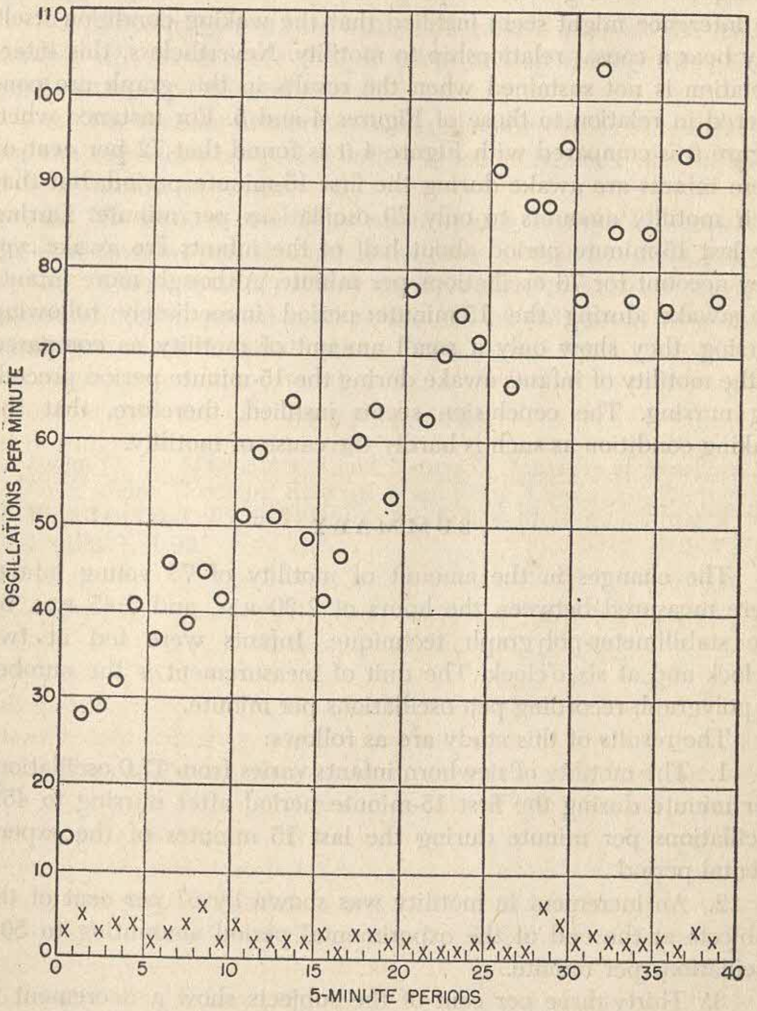

Figure 6. Motility during waking and sleeping (42 cases).

IS THE WAKING STATE A CAUSE OF MOTILITY?

Since there is a striking difference in the amount of motility during the waking and sleeping states as revealed by Figure 6, the inference might seem justified that the waking condition itself may bear a causal relationship to motility. Nevertheless, this interpretation is not sustained when the results in this graph are considered in relation to those of Figures 4 and 5. For instance when Figure 6 is compared with Figure 4 it is found that 72 per cent of these infants are awake during the first 15-minute period, but that their motility amounts to only 20 oscillations per minute. During the last 15-minute period about half of the infants are awake, yet they account for 76 oscillations per minute. Although more infants are awake during the 15-minute period immediately following nursing, they show only a small amount of motility as compared to the motility of infants awake during the 15-minute period preceding nursing. The conclusion seems justified, therefore, that the waking condition as such is hardly the cause of motility.

SUMMARY

The changes in the amount of motility of 73 young infants were measured between the hours of 2:30 P.M. and 5:45 P.M. by the stabilimeter-polygraph technique. Infants were fed at two o'clock and at six o'clock. The unit of measurement is the number of polygraph recording pen oscillations per minute.

The results of this study are as follows:

1. The motility of newborn infants varies from 17.0 oscillations per minute during the first 15-minute period after nursing to 45.0 oscillations per minute during the last 15 minutes of the experimental period.

2. An increment in motility was shown by 67 per cent of the subjects at the end of the experimental period amounting to 50.2 oscillations per minute.

3. Thirty-three per cent of the subjects show a decrement in motility at the end of the period amounting to 16.3 oscillations per minute.

4. Both groups at the beginning of the period show about the same level of motility.

5. The mean increment of motility for the entire group, the specific increment of 49 cases, and the decrement of the 26 cases are statistically significant.

6. When judgments of sleep are made at 15-minute intervals

about 28 per cent of infants were asleep during the first 15 minutes of the experimental period, 63 per cent were asleep during the middle 15-minute period, and 52 per cent were asleep during the last 15 minutes.

7. When judgments are made at 5-minute intervals the largest number of infants were judged to be asleep during the middle third of the experimental period.

8. The amount of motility during sleeping varies little throughout the experimental period either when judgments are made at 5- or 15-minute intervals.

9. The amount of motility when waking during the first part of the experimental period is about the same as that when sleeping.

10. The amount of motility during waking increases throughout the experimental period until at the end of the period it is about nine or ten times its initial value.

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Four—The Effect of Clothing on the General and Vocal Activity of the Newborn Infant

ORVIS C. IRWIN &
LABERTA WEISS

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The immediately preceding study indicated that the infant shows a considerable amount of activity when external conditions remain constant and only internal conditions vary. What happens when different external conditions are imposed?

In previous studies (not included in this volume), Irwin and his co-workers have shown that mild stimulation reduces the activity that occurs in the experimental cabinet. Thus, infants are less active in a lighted cabinet than in a dark one, less active when there is a continuous tone sounding in the cabinet than they are in a quiet cabinet. They are most active when the cabinet is both dark and silent. In the following investigation, Irwin and Weiss show that the presence of clothing reduces activity in a manner similar to the presence of light and sound.

These studies corroborate parental experiences that a restless and crying infant is quieted by mild stimulation—by singing, patting, rocking, or walking the floor. Stronger stimulations such as those derived from a burn or a bruise would, of course, have the opposite effect.

THE PROBLEM

A previous investigation has indicated that light and sound stimuli, when presented over a continuous period, have marked effects upon the activity of newborn infants. The present study represents an application of the previous method to the study of clothing as a determinant of activity in the newborn.

The effect of clothing upon the activity of the newborn infant is a problem of practical and of theoretical significance. The problem involves, first of all, the need for determining whether clothing tends to increase or decrease activity and whether this effect occurs to a significant extent. When once determined, it becomes important to know why the clothing produced its given effect. This question can be answered only through a systematic analysis of various factors: mechanical restraint, temperature change, alleviation of hunger contractions through abdominal pressure, elimination of self-stimulation through prevention of random touching of parts of the body, and so on.

A knowledge of the effect of clothing upon activity is of real significance only when the determining factor as well as the effect is known. If, for instance, clothing is found to decrease activity to a significant extent and if mechanical restraint seems to be the causal factor behind that effect, then one would have an objective basis for believing that such clothing was detrimental to the fullest muscular development of the subject. If, on the other hand, clothing significantly reduced activity but the factor did not prove to be restraint, the above belief would not follow as logically. In such a case it would be necessary to seek further for factors before making any suppositions.

The present study represents only a preliminary investigation of the effect of clothing on the activity of the newborn infant. Since it is preliminary in nature, the primary aim is to determine the nature and extent of the effect of clothing on activity. No specific attempt has been made to determine experimentally the factors back of the given effect, though in the closing sections of the paper secondary information regarding certain of these factors is discussed.

PROCEDURES

The data of the present experiment are of two types: (1) records of general activity and (2) records of crying.

Activity records were collected by means of the stabilimeter-polygraph technique. The activity of each subject was recorded during a six-minute period with clothing and a six-minute period without clothing. The order of these two situations was rotated from one subject to the next.

The crying records consisted of observations at half-minute intervals as to whether the subject was quiet or crying. These data were used to compare trends in crying with polygraph trends in activity.

Control of Extraneous Factors

The subjects were studied in the experimental cabinet similar to that previously illustrated. The degree of illumination maintained throughout the experiment was .02 foot-candle power.

In addition to the factors needing control in the previous studies on vision and sound, an even more important factor was involved in the present investigation, that is, the problem of eliminating disturbances caused by dressing or undressing the infant. The complete elimination of this factor was impossible. The best that could be done was to reduce the disturbance to a minimum.

Each infant, when brought to the experimental room, was placed on the stabilimeter unclothed, with his blanket and diaper beneath him. When the time for the clothing situation arrived, the diaper was pinned together. The experimenter then folded back one corner of a second diaper and placed this bib-fashion around the infant's neck. This second diaper was now in such a position that two ends extended over the infant's arms. These ends were tucked in so as to form a closed sleeve. Finally, the ends of the blanket were pulled around and tucked loosely at the infant's sides. All this gave a result similar to the effect produced by the typical clothing of the infant—which consists of the sleeveless or closed sleeve dress, a diaper, and a blanket—and did not cause the disturbance which actual use of the former articles would have entailed (since a dress has to be slipped over the head and requires lifting the infant to an upright or partially upright position). The significance of the attempt to move the infant in the longitudinal rather than the vertical position and the attempt not to rotate him was to prevent displacement of the viscera and a resultant change in physiological conditions.

RESULTS

The results of the clothing experiment are dealt with under two heads: (1) statistical analyses of the data in the clothed and unclothed situations are given; and (2) the contribution of the data to an understanding of causal factors is discussed.

Five of the infants used in the investigation partially or entirely freed themselves from clothing before the 6-minute period was over. Since inclusion of these cases affects the results on the side of conservatism and since the cases yielded scores which differed in no marked manner from the other cases, they have been included in the following analysis.

Comparison of Clothed and Unclothed Situations

Mean Activity and Variability in the Two Situations. The mean activity of the 50 infants in the clothed and unclothed situations and the variability of those means are presented below:

Situation	*Mean*	*Probable Error of Distribution*	*Probable Error of Mean*
Clothed	18.5	10.3	1.4
Unclothed	34.5	19.6	2.8

The data reveal an average of 16 oscillations per half-minute more in the unclothed than in the clothed situation. In other words, the group was half as active when clothed as when unclothed. The ratio of the difference to the probable error of the difference is 5.16, indicating that the reduction of activity under clothed conditions is of statistical significance.

From the above figures it would also appear that the two situations differed in variability since the probable error of the unclothed situation is numerically twice that of the clothed situation. The marked differences in mean activity between the situations, however, makes this raw comparison invalid. Accordingly, the coefficient of variation has been determined for the two situations. The coefficient for the clothed is 82.2 and for the unclothed 84.1. In other words, there is no difference in variability between the two situations.

Individual Trends. The previous section has indicated that, for the group of 50 infants as a whole, activity was significantly less under clothed than under unclothed conditions. The question remains as to how well this group tendency is supported by the individual cases.

An analysis shows that 47 of the 50 infants in the clothing experiment (94 per cent) revealed trends consistent with the group findings, that is, decreased activity in the clothed as compared with the unclothed situation. There is no apparent reason why the inconsistent cases revealed the reverse trend. Only one of those cases belongs to the group of five infants who freed themselves during the clothed period.

The percentage of agreement noted above is exceedingly high and indicates that the group findings are a true expression of individual trends.

Trend of Activity Curves Within Each Situation. An interesting problem in connection with the present study is the length of time that a given stimulus situation was effective. In answer to that question, the average activity of the 50 infants in each successive half-minute of stimulation was determined for each situation. The computations indicated that the level of activity was different in each situation and that it remained different throughout the six-minute period. In other words, both stimulus situations were effective throughout their duration.

It is possible, however, that there are significant differences in the degree of effectiveness of a given stimulus over a six-minute period. To test this possibility, the mean activity of the infants during the first half of each situation was compared with the mean activity during the last half. The data are presented below:

	First Half of Period		*Last Half of Period*	
Situation	*Mean*	*Probable Error of Mean*	*Mean*	*Probable Error of Mean*
Clothed	13.3	1.5	12.5	1.5
Unclothed	29.3	2.3	39.2	7.2

The ratio of the difference between means to the probable error of the difference is 0.38 in the case of the two clothing means and 1.30 in the case of the two unclothed means. It is clear from these figures that there were no significant changes during the first and last halves of a given period. In other words, activity in each situation was fairly constant in amount over a six-minute interval.

Trend of Crying Data in the Two Situations. Thirty-eight of the 50 infants studied in the clothing experiment cried in either or both the clothed and unclothed situations. Thirty-one infants (82 per cent) cried more in the unclothed than in the clothed period. Five of the subjects (16 per cent) cried more under clothed conditions. The trends revealed by the crying data were substantiated by polygraph records in 95 per cent of the cases.

The above analysis indicates that crying and polygraph trends were very consistent in the present study. This point has an important bearing on the question as to what factors are involved, as will be pointed out later.

CONTRIBUTION OF THE DATA TO AN UNDERSTANDING OF DETERMINING FACTORS

As has already been stated, the present study was not expected to yield an understanding of the factors which make for the differences in activity between clothed and unclothed situations. Nevertheless, it has yielded some suggestive facts. The most important of these bear on the following qeustion: Is the reduction of activity under clothed conditions the result of mechanical restraint, or is it a true differential response evoked by more subtle factors?

Two of the experimental findings suggest the possibility of restraint. One of these is the fact that the range of activity in each successive half-minute of the clothed situation is practically a straight line. These findings are markedly different from those of the visual and auditory experiment and suggest that some new factor is at least operating.

On the other hand, there is much evidence to indicate that restraint is not the only factor operating, if it is effective at all. It should be remembered in this connection (1) that the clothing was loose enough to allow movement, so that hampering could affect only the extent of movement, and (2) that polygraph readings were made so as not to penalize for the extent factor. Furthermore, the fact that the curve over the six-minute period did remain at the same level is proof that clothing did not reduce activity due to a fatigue factor resulting from hampered movement. The fact that the relative variability for the clothed and unclothed situations was almost identical also lessens the significance of the first evidence in favor of restraint factors.

The evidence which crying data add to the present discussion is capable of a two-sided interpretation. It will be remembered that crying was less under clothed than unclothed conditions. Since clothing could not hamper vocal activity even though it might hamper general activity, this would appear to be evidence in favor of a differential as opposed to a restraint factor.

It is evident from the above discussion that an understanding of the causal factors behind the quieting effect of clothing is far from simple. It will take a systematic investigation of each possibility to solve the problem adequately.

SUMMARY

The overt activity and the amount of crying of 50 newborn infants were studied in clothed and unclothed situations for the purpose of determining the effect of clothing upon the infants' behavior. The study showed:

1. The group as a whole was significantly less active when clothed than when unclothed.

2. Ninety-four per cent of the individual subjects were quieter in the clothed than in the unclothed situation.

3. The reduction in activity in the clothed situation was maintained rather uniformly throughout the period of stimulation.

4. Crying was also less in the clothed than in the unclothed period.

Five—The Problem of Conditioning in the Neonate

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When is a human being first capable of learning? The answer to this question depends in part upon one's definition of learning. One of the most studied types of learning is the conditioned response. Several studies of newborn infants, and even studies of fetuses in utero, have apparently shown that the older fetus and the neonate can be conditioned to respond to previously ineffective stimuli. However, the following investigation shows that new stimuli may become effective even without the usual conditioning procedure. Some modification of behavior occurs but it does not fit the pattern of the classical conditioned response. We need to know much more about the ways in which the young infant's behavior can be modified and about the degree of permanence of these modifications.

PROBLEM

The purpose of the present experiment was to investigate the problem of the conditionability of a withdrawal movement in the newborn infant, and, if positive results were obtained, to study other conditioning phenomena such as experimental extinction and spontaneous recovery in these subjects. This general problem has already been attacked in similar subjects by Marquis (4), who used sucking as her unconditioned response, and by Wenger (8), who employed lid closure and the withdrawal of the leg to shock in addition to sucking as a response. Although both of these studies obtained results which the writers interpreted as positive, neither of them is entirely conclusive and both have been subjected to criticism. Pratt (5) and others (8) have suggested that Marquis has not clearly shown that the response to the conditioned stimulus which she obtained after her training series might not have occurred without this training. Further, Wenger, using a procedure similar to that of Marquis, obtained negative results for this particular response. Wenger's own study, although careful and approaching objectivity, must be criticized because of the limited number of subjects used; in many groups only two or three infants were used and one important control group consisted of but four subjects. Since there is a considerable overlapping between the results of his control and his experimental group, this limitation in the number of subjects becomes a serious criticism. A further consideration of this point will be given in discussion of our results.

Because of these objections, it was felt that an additional experiment might clarify the question as to whether or not the newborn infant can become conditioned.

PROCEDURES

Subjects were under-ten-day infants born in the Ohio State University Hospital. Thirty-six infants in all were used in the actual experiment. These were full-term babies about equally divided as to sex. Approximately one-third of the subjects were of at least partial Negro parentage and the remainder were of white percentage. The infants were from three to five days old when they were brought in for the first experimental session, and, the study continuing for four days, the subjects were from seven to nine days of age at the last experimental period.

All experimentation was done with the infants inclosed in the soundproof, thermostatic isolation cabinet, described by Dockeray and Valentine.* (2)

Before the formal experimentation on the 36 final subjects was begun, various sorts of stimuli were administered to a number of other infants with a view to determining the optimal unconditioned stimulus. From these investigations it was concluded that the best type of stimulus for the study of conditioning of a withdrawal movement was a shock to the sole of the foot. To this stimulus the infant usually responds bilaterally with flexor movements of legs, feet, and toes, often followed by extensor movements. This response is usually more marked in the member receiving the shock than it is in the other member. Shock then was the conditioned stimulus. This was delivered from a Harvard inductorium, the current being derived from a 6 volt storage battery. The electrode consisted of two 4 by 36 bolt heads attached to the surface of a flexible rubber disc about the size of a penny. This was attached to the sole of the right foot by a strip of Sealtex tape. The conditioned stimulus was the sound of a muffled buzzer, placed in the cabinet about two feet from the infant's head. The buzzer and shock were administered by hand, with the buzzer (in the paired stimulation) preceding the shock by about a fourth of a second and continuing about a fourth a second more, along with the shock. This hand method of giving stimulations was, of course, necessarily subject to some variability. Stimulations were spaced one, two, three, or four minutes apart according to a varied and irregular pattern. An attempt was made to stimulate the infant only when his activity level corresponded with stages three to five of Wagner's criteria of sleep (7), that is, when it was neither crying nor highly active and yet not in an extremely deep sleep. Since the babies did not always cooperate, the time intervals between the presentations of the paired stimuli occasionally ran over the one- to four-minute figures mentioned above. The same infant was always run at the same time on succeeding days, the times being directly following either the two or four o'clock feeding periods. The two experimenters observed and recorded the responses of the infants, one noting the movements of one limb and one the movements of the other. The responses were recorded upon form sheets in terms of flexor and extensor movements of the leg, foot, and toes.

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* This cabinet is similar to the one illustrated on page 21.

Three separate groups, consisting of 12 infants each, were used —an experimental group and two controls. For all three groups items of initial procedure were the same. The infant was placed in the isolation cabinet, the electrode having been attached to the sole of the right foot. It was allowed about five minutes to become adjusted to the situation; then tests for responses to the buzzer alone were administered. Three different stimulations of the sound of the buzzer were given, and, if the infant responded by any movement of leg, foot, or toe, he was discarded and not used in any of the three groups. From this point on, the procedure for the different groups varied.

The experimental group received 36 paired stimulations of buzzer and shock, 12 each day for three consecutive days. At the end of the third day, the buzzer alone was sounded to test for the presence of conditioning, and these stimulations of the buzzer were administered until there were three consecutive failures to respond—

the criterion of extinction in this experiment. On the fourth day, the buzzer alone was administered until the response was extinguished. This was done, of course, to test for the spontaneous recovery of the previously extinguished conditioned response.

In view of the possibility that positive results in the above group, if present, might be due to the effect of repeated presentations of shock, the unconditioned stimulus, rather than to the pairing of the buzzer and shock, the following control group was set up, which we shall call Control Group 1. Following the usual tests to the buzzer alone, the 12 infants of this group were given 36 shocks alone, 12 each day, for three consecutive days. Thus, they never received a paired stimulation of shock and buzzer. At the end of this period, on the third and fourth days, the buzzer alone was sounded, tests for extinction and spontaneous recovery of the response, if any, being administered just as in the experimental group.

Control Group 2 was run to check the possibility that any responses to the buzzer on the third day might be due solely to the factor of maturation rather than to training. The procedure for this group consisted in testing for responses to the buzzer alone on day 1; then, on day 3, after two days' delay, during which time no stimulations were given by the experimenters, administering a second test for responses to the buzzer alone.

RESULTS

The results for the experimental group are presented in Table I. Each baby is listed separately. Day 3 is, of course, the time of the test for presence of the conditioned response, following the training series of 36 paired stimulations; day 4 was the day of tests for spontaneous recovery of the response which had been extinguished on the previous day. By number of responses is meant the number of times the infant responded to the buzzer alone by some movement

TABLE I

Showing the Number of Responses to the Buzzer Alone for the Experimental Group on the Third and Fourth Days of the Experiment, Day 3 Being the Original Test for the Presence of Conditioning, and Day 4 Being a Test for Spontaneous Recovery

Number of Responses	*Subjects*											
	He	Ca	De	Ch	Re	Mc	Bo	Jo	Ot	Fo	Gr	Ma
Day 3	2	2	4	0	3	1	5	2	0	1	0	3
Day 4	8	9	–	0	4	10	7	1	8	1	0	0

of both limbs,* that is, gave a conditioned response. For example, subject He gave two conditioned responses on the third day and eight responses on the fourth day. It will be seen that 9 of the 12 infants in the experimental group gave conditioned responses to the buzzer alone on the third day. Only 11 of the 12 subjects were tested for spontaneous recovery. Of those who showed conditioning on the previous day, nine showed spontaneous recovery, and one infant, who gave no conditioned responses on the previous day, gave several such responses on the spontaneous-recovery day. In general, then, the results of the experimental group indicate that conditioning of the withdrawal response, its extinction, and the spontaneous recovery of the response on the following day, are obtainable in the under-ten-day-old infant.

The clearness of this picture is, however, clouded by the findings of the first control group, for the behavior of these subjects was practically identical with that of the experimental group, in spite of the fact that the subjects of Control Group 1 had never had a paired presentation of buzzer and shock. The results for Control Group 1 will be found in Table II, and may be read in the same manner as Table I. Ten out of the 12 infants responded to the buzzer, after the 36 shocks alone; all of these showed extinction, and of the 10 babies who were tested on the fourth day, 8 showed spontaneous recovery. Needless to say, the slight differences between this control group and the experimental are not statistically significant. Conditioning, if you will, extinction, and spontaneous recovery are just as clearly demonstrated in this group as in the previous one.

TABLE II

SHOWING THE NUMBER OF RESPONSES TO THE BUZZER ALONE FOR CONTROL GROUP 1 ON THE THIRD AND FOURTH DAYS OF THE EXPERIMENT

Number of Responses	*Subjects*											
	Hi	Al	Ad	Pa	Ja	Th	En	Sm	Am	Kl	Fj	Wi
Day 3.........	2	1	0	2	3	2	5	2	3	2	2	1
Day 4.........	2	10	0	9	3	2	3	–	5	–	2	1

It immediately occurred to the experimenters that these results might be due to faulty mechanics in the experimental setup. It might be possible that, although the infant was enclosed in a supposedly soundproof cabinet, he was able to react to the sound of the vibrator on the inductorium itself, and this might become a conditioned stimulus which could be easily generalized to the buzzer. That this was not true was proved by including checks brought about by activating the inductorium alone without the application of shock. In no cases were any responses made to these checks.

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* Movement of both limbs was demanded as a criterion of the conditioned response since this bilateral movement was typical of the response to the unconditioned stimulus.

There still remained the possibility that the responses to the buzzer alone given on the third and fourth days were due simply to maturational factors (that is, increase in auditory acuity) rather than to any effects of the experimental routine. As will be recalled, Control Group 2 was designed to investigate this possibility. This group simply received two tests for responses to the buzzer, separated by two days. Of course, as in the other groups, no subject was included who responded to the buzzer on the first day. Only one out of the 12 subjects used gave any response to the buzzer on the second test. Applying the Chi Square formula we find that there is a statistically significant difference between this group and the experimental group and between this group and Control Group 1.

At this point it would perhaps be in order to consider the only other study we have found which investigated conditioning of the withdrawal movement in the under-ten-day-old infant, at the same time employing a control group similar to our Control Group 1. Wenger (8), using five infants in his experimental group, concluded that the movement of withdrawal from shock in the infant could be conditioned to a tone. He ran a control of four subjects in which shock alone was given, followed by tests for responses to the tone (without any pairing of conditioned and unconditioned stimulus). This control group did give a number of responses to the tone alone. However, because the experimental group gave a few more such responses, Wenger concludes that there were essential differences between the two groups. An examination of his tables, however, indicates that this is not necessarily true, that there is no statistically significant difference between his experimental and control groups, and that his results are actually in accord with ours. Whatever differences there exist between the two groups may be attributed to sampling errors.

DISCUSSION

There seems, then, to be an essential similarity between our experimental group and our first control group, both of them showing evidences of "conditioning," extinction of the conditioned response, and spontaneous recovery, even though the situation for the control group was not a conventional conditioning situation. This similarity seems not to be due to artifacts of the experimental setup, or to any maturational effects. There remains the problem of accounting for it. Two possible interpretations present themselves. These are highly speculative, neither proved nor disproved by the extant experimental evidence. However, they are perhaps worthy of consideration with a view to further investigation.

1. Perhaps the continued stimulation which results in the leg movement characteristic response to shock serves to sensitize and make this response prepotent. The muscles involved in the response are, then, in a state of readiness, and may be tripped off at almost any stimulation. Highly analogous to this in the field of infant behavior are the findings of Pratt, Nelson, and Sun (6), Jensen (3), and others, which indicate that nearly any stimulus will set off the sucking response, supposedly a prepotent response in the neonate. Possibly analogous in the field of adult conditioning is the phenomenon of sensitization (Bernstein, 1). If this interpretation of sensitization is accepted, it would seem that our infants are not showing conditioning in the ordinary meaning of the term. However, before sensitization can become a complete explanation, we must know more concerning the nature of this phenomenon. The fact that spontaneous recovery existed—that these withdrawal responses to the buzzer appeared as late as 24 hours after the last shock, sometimes indeed with greater frequency than in the tests immediately following shock—certainly implies that sensitization is a relatively long-lasting thing.

2. Perhaps conditioning in the ordinary sense is operating after all, but we, as experimenters, are not clearly recognizing the nature of the conditioned stimulus. Perhaps the infant, in responding to the conditioned stimulus, is not responding to the sound of the buzzer as such, but merely to the buzzer's characteristic of being a sudden change in the environment. If we analyze the nature of the shock, we may see how the infant has become so conditioned, that is, conditioned to respond to a sudden change in the environment. The shock has two characteristics: it is not alone a pain-giving and response-producing stimulus, but it also possesses the character of being a sudden, sharp change in the environment. That is, first there is no shock; then, suddenly, there is shock. This sudden change in the environment is the conditioned stimulus, and it is always followed by pain, the unconditioned stimulus. Thus, the organism becomes conditioned to a change in the environment, and the buzzer, which is also a change in the environment, may now act as a conditioned stimulus. If this were true, we would expect many other stimuli which are effective to the infant also to set off the conditioned response, and this, of course, could easily be attacked experimentally.

The interpretation implies that the infant does not easily make discriminations between specific stimuli, and reacts instead to major changes and characteristics—a view which is also supported by the findings of Pratt, Nelson, and Sun (6).

These theoretical interpretations may be of some value in suggesting problems for future attack. In any case, our data seem to have reopened the question of conditionability of the newborn infant. The results would indicate that, without further experimentation, it cannot be stated that the withdrawal movement in the neonate can be conditioned, in the ordinary sense of that term. Modifiability, however, is clearly shown, and conditioning of some sort may be operating. The investigation points incidentally to the need for adequate controls in conditioning experiments, especially in those which use lower organisms as subjects.

SUMMARY

To investigate the problem of conditionability of the neonate, three groups, consisting of 12 under-ten-day-old infants, were studied, the conditioned stimulus being the sound of a buzzer, the unconditioned stimulus a shock to the sole of the foot.

Members of the experimental group received 12 paired stimulations a day for three consecutive days. They were then tested for conditioned responses to the buzzer alone, the responses were extinguished, and on the following day tests for spontaneous recovery were made. Members of Control Group 1 received 12 shocks alone per day for three consecutive days. Like members of the experimental group, they were tested for responses to the buzzer alone at the end of the third day and on the fourth day. Members of Control Group 2 were tested for responses to the buzzer alone on the first day; then, two days later, during which time no stimulations were given, they were again tested for responses to the buzzer alone.

Results showed that the experimental group gave evidence of conditioning, extinction, and spontaneous recovery. Control Group 1 gave almost identical results, although members of this group had never had a paired presentation of conditioned and unconditioned stimulus. Only one subject in Control Group 2 responded to the buzzer alone on the third day.

Conditionability (in the ordinary sense of the term) of the neonate, and incidentally also of the fetus, is thus not unequivocably supported. Two possible theoretical interpretations are advanced.

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Six—Is the Newborn Infant's Repertoire Learned or Unlearned?

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The author's view is that in the early phases of human development, learning plays little or no rôle in the growth of behavior. The rôle of learning becomes increasingly important as the child grows older, since with age he becomes much more able to learn. The selection that follows should, therefore, be thought of as treating the place of learning in only one phase of growth. Later selections, particularly those in Chapter III, will deal with learning at later ages.

Common sense sees a large unlearned element in the behavior of the newborn infant, since the opportunities for learning in utero are obviously limited. However, from time to time, it has been indicated that the possibility of learning is not entirely absent prior to birth, and some writers have gone so far as to suggest that the neonate's repertoire in its entirety may have been learned during the fetal period.

Among the most understanding of the latter group is Holt. In *Animal Drive and the Learning Process* (9) he very cleverly suggests mechanisms whereby responses, even reflexes, might be learned during the period of gestation. His treatment of the grasp reflex is the most striking case. He notes that whenever the muscles of the hand are flexed, from whatever cause, the skin of the palm is stimulated. He argues that after several self-stimulations of the palm have occurred in association with hand closure, stimulation of the palm becomes sufficient to elicit the grasp reflex. The same theory has been presented by Givler (6) and Hollingworth (8).

PRE-NATAL OPPORTUNITIES FOR LEARNING

Such a theory, of course, makes use of the easiest instance. Had the congenital stimulus for the grasp reflex been contact with the *back* of the hand or with the forearm instead of stimulation of the palm, the task of the theorist who wished to account for the neonatal stimulus in terms of learning would not have been so easy. When reflexes other than the grasp reflex are examined, serious difficulties are encountered with an interpretation based upon fetal conditioning.

It will be instructive to consider the possibility of learning the knee-jerk. Here the theory which accounted for the grasp reflex fails to apply. Postnatally the adequate stimulus is a blow applied quickly and forcibly above or below the patella. According to Holt's theory, movement of the leg ought to be the stimulus to the knee-jerk, since the knee-jerk stimulates proprioceptors. Actually, movement of the leg does not cause a knee-jerk.

Similarly, if the knee-jerk were elicited in utero, another consequence would be that the propulsion of the first toe against the enclosing membranes. In this instance, according to Holt's theory, the neonatal stimulus to the knee-jerk should be not a blow applied below the knee cap but a blow applied to the end of the big toe. A sweeping application of the conditioned reflex theory to the problem of fetal development must take into account the fact that some stimuli do not bring the results which would be expected on the basis of the learning hypothesis. One is forced to conclude that the grasp reflex is merely an instance in which the facts accidentally fit the learning hypothesis. If the hypothesis were correct, all other instances should fit it equally well, and this they do not do.

The greatest weakness of the theory of development through learning lies in the fact that in regard to most aspects of neonatal behavior the possibilities of intrauterine learning do not seem to exist. This can be illustrated with regard to several classes of responses. Among the response mechanisms which are best coordinated at birth are the visuo-ocular responses. In the newborn the pupillary response is present, and it is consensual to monocular stimulation. Eyelid response also is present. When the newborn infant is awake in dim or in moderate light the lids are open; they are closed in response to intense illumination. These eyelid responses also are coordinated. In some newborn infants visual fixation and good degree of visual pursuit are present; in others, the responses are present but are imperfect. In practically all infants the movements of the two eyes are coordinated (4).

The responses just described indicate briefly the extent of the congenital visual-motor coordinations. Now there are no reasons to believe that light stimulation occurs prenatally. How can a learning theory account for the existence at birth of complicated reactions to visual stimuli, if light stimulation has never taken place prior to birth?

Such questions could be multiplied many times. To cite further instances, the following problems may be proposed: What is the possibility of learning to cry before breathing takes place? How could crying have been associated, through experience, to the numerous stimuli which are encountered only after birth and which instigate crying upon their first presentation, such as heat, cold, intense tastes, electric shock?

Can sneezing occur during the gestation period? The Moro reflex? If they cannot, how can these motor patterns or their stimulus response connections be learned? Some infants at birth can balance the head when held upright. In the case of this reaction, as in the case of many others, the restraint of the fetal state renders the prenatal practice of such a response impossible.

It cannot be denied that there *are* responses to which a intrauterine learning theory seems applicable. The grasp reflex does not stand alone. Many responses are elicitable before birth, and it is possible to urge that some response patterns and their stimulating conditions may be affected by these prenatal elicitations. Among such responses are sucking, swallowing, turning the head, moving the limbs. Let us now examine the question as to whether the performance of these responses before birth leads to the formation of any conditioned responses.

RELEVANCE OF CONDITIONING STUDIES

In this connection studies by Marquis (15, 16), Wenger (24), Wickens and Wickens (25), and others (11, 12) on the possibility of conditioning the neonate immediately come to mind. We need to ask whether or not experiments on neonatal conditioning have any bearing upon the question of fetal behavioral development. We think they do not. The possibility of early postnatal training should not be confused with the question as to whether or not conditioned responses are formed under the normal uterine conditions which precede birth. The experiments on conditioning in the neonate necessarily deal with artificial conditions, whose similarity to intrauterine conditions is problematical.

Classical conditioning, as an experimental procedure, consists of a training sequence whereby new stimuli are made effective. Even if this result could readily be attained in the fetus, it is doubtful that the process could account for an appreciable part of infant behavior. Conditioning, defined in the terms outlined above, is not known to be able to account for *new patterns* of behavior. It can be called into play as an explanatory concept only in regard to the establishment of the stimulus control of responses, not for the creation of new modes of behavior. It is true that according to some theories the formation of response patterns may be accounted for by the laws of the conditioned reflex. These views, however, are theories which are not as yet verified, and they do not offer a secure basis for the learning hypothesis of fetal development.

It is important to note that operant conditioning and trial-and-error learning have not yet been demonstrated either in the newborn infant or in the fetus.

It is difficult to perform experiments in human learning prior to birth. For this reason, the study of early learning has been, and probably will be, concentrated largely in the neonatal period. There is no desire on our part to deprecate such research. The remarks which we have made above are not a general criticism of experiments upon learning in the neonate; they are simply comments concerning the relevance of such studies to the question of the development of fetal behavior. It is recognized that not many experimenters would propose that researches upon learning in the newborn are relevant to the topic of the growth of fetal behavior. Nevertheless it needs to be emphasized that the demonstration of learning in the newborn cannot establish the view that learning constitutes the basis for the prenatal growth of behavior.

Many human responses are capable of elicitation not only at birth, but long before the normal end of gestation. The infant born after eight months' gestation is not lacking in any of the responses present in the infant of normal term, although the responses of the infant which is one month premature may be slower, and less forceful, and less well coordinated. Practically the same thing may be said of the behavior of the child born at seven months. The nursing reflex may be weak, but it is present. All of the usual forms of neonatal behavior may be elicited (19). It is obvious, therefore, that the fundamental developmental processes take place long before birth. In other words, the formation of conditioned responses at birth does not show that what is elicitable at birth has been learned, since most of the congenital responses are demonstrable at a period which antedates by at least two months the period of birth.

Indeed, in order to explain the behavioral development of the fetus in terms of learning, the fetus would have to be proved to be capable of learning long before the seven-month stage is reached. This is shown by Hooker's study of the behavioral development of the fetus. His investigation shows at what an early period some behavior originates, and reveals to what extent a learning hypothesis of behavioral development must be projected into the prenatal period, if it is to account for all congenital responses.

NEUROLOGICAL IMMATURITY AT BIRTH

Attention must be directed also to the neurological condition of the fetus and the neonate. It has long been known that the behavior of the neonate is relatively normal even when the entire cerebrum is absent. Dating at least as early as the beginning of the seventeenth century, there have been numerous reports of monstrous human births congenitally lacking the cerebrum, or lacking even a larger portion of the brain. While these anencephalies are usually stillborn or very short-lived, they may live for several minutes or even for several hours, during which time their behavior may be observed. In referring to the behavior of acephalies, it must be kept in mind that few of the observers were especially interested in behavior and for this reason the account of the responses of each acephaly is probably incomplete. Nevertheless, it is worth while to examine these records. The acephaly observed by Lallemand (13), cried, breathed, sucked, and gave the grasp reflex. Spezza's case (22) breathed, cried, and moved its limbs. Putnam's account (21) of an acephaly shows that it breathed well, cried, sucked, swallowed; there were occasional movements of the limbs and of the eyelids. This creature possessed no nervous structures above the medulla. Preyer (20) described an acephalous child, entirely without a cerebrum, which cried, sucked, swallowed, grasped, yawned, thrust out its tongue, turned its head from side to side, and sneezed. The child reported by Angell and Elsner (1) "appeared able to direct its gaze to a limited extent, a lighted candle holding its attention." It gave a slight pupillary reflex.

If all of the neonatal responses now known (4) had been tested in these acephalies, it is likely that most of them could have been elicited. As it is, it is clear from these cases that many response patterns of the newborn need not involve the cerebrum, a conclusion which has been drawn from data such as those cited above by many writers including Marshall Hall (7) and Preyer (20). Experimental procedures by Minkowski (17) and Bolaffio and Artom (2) likewise have revealed that the cerebrum exercises little or no control over fetal behavior.

These facts present another serious obstacle to a learning hypothesis of fetal behavioral development. It is true that recent experiments (3) have shown, contrary to Pavlov's claim, that conditioning can occur in decerebrate preparations. Nevertheless, such conditioning is difficult to obtain, and it differs in several respects from conditioning involving the cortex. The cortex is certainly the structure whose presence makes learning most easy. To suppose that an organism in which the cortex is practically nonfunctional develops behavior through learning rather than through neural growth means the acceptance of a difficult hypothesis for which there is no supporting evidence.

It was mentioned earlier that acephalies ordinarily live but a few hours or a few days. Occasionally, however, they do live much longer. Most outstanding in this connection is the case, reported by Gamper (5), which lived two and one-half years. During this period bodily growth was fairly normal, and the child was in good health, *but no behavioral advance took place.* This would seem to be a crucial case for the learning theory. If prenatal development occurs by virtue of a sort of subcortical learning, one wonders why such learning did not continue to occur in this acephalous infant. On the other hand, if behavior develops through neural growth, as the maturation theory holds, it would not be expected that this child would show any postnatal behavioral development since there is no reason to believe that any neural growth took place during its postnatal existence.

In further support of this view is the case reported by Talbot (23), an acephaly which lived for eight months without exhibiting any behavioral development.

That fetal responses are learned is a hypothesis that requires careful analysis. Our examination of the question leads us to believe, despite the ingenious suggestions of Holt (9) and Hollingworth (8), that most of the behavior of the infant at birth is unlearned, and that fetal development is almost entirely a matter of maturation.

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II–DEVELOPMENT IN THE FIRST YEARS

One—A Biographical Sketch of an Infant

CHARLES DARWIN

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The earliest accounts of psychological development were records of individual children. The first of these was published by Tiedemann in 1787. Several such records were kept during the first three quarters of the nineteenth century but the publication of them did not become common until after 1880. It is interesting to note that Darwin started the following record of the development of one of his children in 1840 but did not publish it until 1877.

Each biographer is likely to reveal his own interests in his observations; some biographers are interested primarily in the senses, some are concerned chiefly with motor development, and others give most attention to the growth of language. Darwin is clearly a biologist even when observing infants; he makes several comparisons between infants and the young of other species. Indeed, as he indicates, one of his chief motives for the observation of infants was to gather comparative material for his book THE EXPRESSION OF EMOTIONS IN ANIMALS AND MAN.

Biographical studies have the advantage of showing in detail how an INDIVIDUAL *child develops, something one cannot learn from a set of norms and averages. To some extent, the reading of biographies of children can compensate for a student's lack of direct contact with children. For the student's convenience a bibliography of child biographies is placed at the end of Darwin's record. The student will find, however, that most of the significant findings in child psychology, as illustrated in the selections in this volume, could not have been derived from biographies.*

M. Taine's very interesting account of the mental development of an infant, translated in the last number of *Mind*, has led me to look over a diary which I kept 37 years ago with respect to one of my own infants. I had excellent opportunities for close observation, and wrote down at once whatever was observed. My chief object was emotional expression, and my notes were used in my book on this subject; but as I attended to some other points, my observations may possibly possess some little interest in comparison with those by M. Taine, and with others which hereafter no doubt will be made. I feel sure, from what I have seen with my own infants, that the period of development of the several faculties will be found to differ considerably in different infants.

SENSITIVITY AND MOVEMENT

During the first seven days various reflex actions, namely sneezing, hiccoughing, yawning, stretching, and of course sucking and screaming, were well performed by my infant. On the seventh day, I touched the naked sole of his foot with a bit of paper, and he jerked it away, curling at the same time his toes, like a much older child when tickled. The perfection of these reflex movements shows that the extreme imperfection of the voluntary ones is not due to the state of the muscles or of the coordinating centers. At this time, though so early, it seemed clear to me that a warm soft hand applied to his face excited a wish to suck. This must be considered as a reflex or an instinctive action, for it is impossible to believe that experience and association with the touch of his mother's breast could so soon have come into play. During the first fortnight he often started on hearing any sudden sound, and blinked his eyes. The same fact was observed with some of my other infants within the first fortnight. Once, when he was sixty-six days old, I happened to sneeze, and he started violently, frowned, looked frightened, and cried rather badly: For an hour afterwards he was in a state which would be called nervous in an older person, for every slight noise made him start. A few days before this same date, he first started at an object suddenly seen; but for a long time afterwards sounds made him start and wink his eyes much more frequently than did sight; thus when one hundred and fourteen days old, I shook a pasteboard box with comfits in it near his face and he started, whilst the same box when empty or any other object shaken as near or much nearer to his face produced no effect. We may infer from these several facts that the winking of the eyes, which manifestly serves to protect them, had not been acquired through experience. Although so sensitive to sound in a general way, he was not able even when one hundred and twenty-four days old easily to recognize whence a sound proceeded, so as to direct his eyes to the source.

With respect to vision—his eyes were fixed on a candle as early as the ninth day, and up to the forty-fifth day nothing else seemed thus to fix them; but on the forty-ninth day his attention was attracted by a bright-colored tassel, as was shown by his eyes becoming fixed and the movements of his arms ceasing. It was surprising how slowly he acquired the power of following with his eyes an object if swinging at all rapidly; for he could not do this well when seven and a half months old. At the age of thirty-two days he perceived his mother's bosom when three or four inches from it, as was shown by the protrusion of his lips and his eyes becoming fixed; but I much doubt whether this had any connection with vision; he certainly had not touched the bosom. Whether he was guided through smell or the sensation of warmth or through association with the position in which he was held, I do not at all know.

The movements of his limbs and body were for a long time vague and purposeless, and usually performed in a jerking manner; but there was one exception to this rule, namely, that from a very early period, certainly long before he was forty days old, he could move his hands to his own mouth. When seventy-seven days old, he took the sucking bottle (with which he was partly fed) in his right hand, whether he was held on the left or right arm of his nurse, and he would not take it in his left hand until a week later although I tried to make him do so; so that the right hand was a week in advance of the left. Yet this infant afterwards proved to be left-handed, the tendency being no doubt inherited—his grandfather, mother, and a brother having been or being left-handed. When between eighty and ninety days old, he drew all sorts of objects into his mouth, and in two or three weeks' time could do this with some skill; but he often first touched his nose with the object and then dragged it down into his mouth. After grasping my finger and drawing it to his mouth, his own hand prevented him from sucking it; but on the one hundred and fourteenth day, after acting in this manner, he slipped his own hand down so that he could get the end of my finger into his mouth. This action was repeated several times, and evidently was not a chance but a rational one. The intentional movements of the hands and arms were thus much in advance of those of the body and legs; though the purposeless movements of the latter were from a very early period usually alternate as in the act of walking.

When four months old, he often looked intently at his own hands and other objects close to him, and in doing so the eyes were turned much inwards, so that he often squinted frightfully. In a fortnight after this time (that is, at one hundred and thirty-two days old) I observed that if an object was brought as near to his face as his own hands were, he tried to seize it, but often failed; and he did not try to do so in regard to more distant objects. I think there can be little doubt that the convergence of his eyes gave him the clue and excited him to move his arms. Although this infant thus began to use his hands at an early period, he showed no special aptitude in this respect, for when he was two years and four months old, he held pencils, pens, and other objects far less neatly and efficiently than did his sister who was then only fourteen months old, and who showed great inherent aptitude in handling anything.

ANGER

It was difficult to decide at how early an age anger was felt; on his eighth day he frowned and wrinkled the skin round his eyes before a crying fit, but this may have been due to pain or distress, and not to anger. When about ten weeks old, he was given some rather cold milk and he kept a slight frown on his forehead all the time that he was sucking, so that he looked like a grown-up person made cross from being compelled to do something which he did not like. When nearly four months old, and perhaps much earlier, there could be no doubt, from the manner in which the blood gushed into his whole face and scalp, that he easily got into a violent passion. A small cause sufficed; thus, when a little over seven months old, he screamed with rage because a lemon slipped away and he could not seize it with his hands. When eleven months old, if a wrong plaything was given him, he would push it away and beat it; I presume that the beating was an instinctive sign of anger, like the snapping of the jaws by a young crocodile just out of the egg, and not that he imagined he could hurt the plaything. When two years and three months old, he became a great adept at throwing books or sticks, and the like at anyone who offended him; and so it was with some of my other sons. On the other hand, I could never see a trace of such aptitude in my infant daughters.

FEAR

This feeling probably is one of the earliest which is experienced by infants, as shown by their starting at any sudden sound when only a few weeks old, followed by crying. Before the present one was four and a half months old I had been accustomed to make close to him many strange and loud noises, which were all taken as excellent jokes, but at this period I one day made a loud snoring noise which I had never done before; he instantly looked grave and then burst out crying. Two or three days afterwards, I made through forgetfulness the same noise with the same result. About the same time (on the one hundred and thirty-seventh day) I approached with my back toward him and then stood motionless: he looked very grave and much surprised, and would soon have cried, had I not turned round; then his face instantly relaxed into a smile. It is well known how intensely older children suffer from vague and undefined fears, as from the dark, or in passing an obscure corner in a large hall, and so on. I may give as an instance that I took the child in question, when two and a quarter years old, to the Zoological Gardens, and he enjoyed looking at all the animals which were like those that he knew, such as deer, antelopes, and so on, and all the birds, even the ostriches, but was much alarmed at the various larger animals in cages. He often said afterwards that he wished to go again, but not to see "beasts in houses"; and we could in no manner account for this fear.

PLEASURABLE SENSATIONS

It may be presumed that infants feel pleasure whilst sucking, and the expression of their swimming eyes seems to show that this is the case. This infant smiled when forty-five days, a second infant when forty-six days old; and these were true smiles, indicative of pleasure, for their eyes brightened and eyelids slightly closed. The smiles arose chiefly when looking at their mother, and were therefore probably of mental origin; but this infant often smiled then, and for some time afterwards, from some inward pleasurable feeling, for nothing was happening which could have in any way excited or amused him. When one hundred and ten days old he was exceedingly amused by a pinafore being thrown over his face and then suddenly withdrawn; and so he was when I suddenly uncovered my own face and approached his. He then uttered a little noise which was an incipient laugh. Here surprise was the chief cause of the amusement, as is the case to a large extent with the wit of grown-up persons. I believe that for three or four weeks before the time when he was amused by a face being suddenly uncovered, he received a little pinch on his nose and cheeks as a good joke. I was at first surprised at humor being appreciated by an infant only a little above three months old, but we should remember how very early puppies and kittens begin to play. When four months old, he showed in an unmistakable manner that he liked to hear the pianoforte played; so that here apparently was the earliest sign of an esthetic feeling, unless the attraction of bright colors, which was exhibited much earlier, may be so considered.

AFFECTION

This probably arose very early in life, if we may judge by his smiling at those who had charge of him when under two months old; though I had no distinct evidence of his distinguishing and recognizing anyone, until he was nearly four months old. When nearly five months old, he plainly showed his wish to go to his nurse. But he did not spontaneously exhibit affection by overt acts until a little above a year old, namely, by kissing several times his nurse who had been absent for a short time. With respect to the allied feeling of sympathy, this was clearly shown at six months and eleven days by his melancholy face, with the corners of his mouth well depressed, when his nurse pretended to cry. Jealousy was plainly exhibited when I fondled a large doll, and when I weighed his infant sister, he being then fifteen and a half months old. Seeing how strong a feeling jealousy is in dogs, it would probably be exhibited by infants at an earlier age than that just specified, if they were tried in a fitting manner.

ASSOCIATION OF IDEAS; REASON

The first action which exhibited, as far as I observed, a kind of practical reasoning, has already been noticed, namely, the slipping his hand down my finger so as to get the end of it into his mouth; and this happened on the one hundred and fourteenth day. When four and a half months old, he repeatedly smiled at my image and his own in a mirror, and no doubt mistook them for real objects; but he showed sense in being evidently surprised at my voice coming from behind him. Like all infants he much enjoyed thus looking at himself, and in less than two months perfectly understood that it was an image; for if I made quite silently any odd grimace, he would suddenly turn round to look at me. He was, however, puzzled at the age of seven months, when being out of doors he saw me on the inside of a large plate-glass window, and seemed in doubt whether or not it was an image. Another of my infants, a little girl, when exactly a year old, was not nearly so acute, and seemed quite perplexed at the image of a person in a mirror approaching her from behind. The higher apes which I tried with a small looking-glass behaved differently; they placed their hands behind the glass, and in doing so showed their sense, but far from taking pleasure in looking at themselves they got angry and would look no more.

When five months old, associated ideas arising independently of any instruction became fixed in his mind; thus as soon as his hat and cloak were put on, he was very cross if he was not immediately taken out of doors. When exactly seven months old, he made the great step of associating his nurse with her name, so that if I called it out he would look round for her. Another infant used to amuse himself by shaking his head laterally: we praised and imitated him, saying "Shake your head"; and when he was seven months old, he would sometimes do so on being told without any other guide. During the next four months the former infant associated many things and actions with words; thus when asked for a kiss he would protrude his lips and keep still—would shake his head and say in a scolding voice "Ah" to the coal-box or a little spilt water, and so on, which he had been taught to consider as dirty. I may add that when a few days under nine months old he associated his own name with his image in the looking-glass, and when called by name would turn toward the glass even when at some distance from it. When a few days over nine months, he learnt spontaneously that a hand or other object causing a shadow to fall on the wall in front of him was to be looked for behind. Whilst under a year old, it was sufficient to repeat two or three times at intervals any short sentence to fix firmly in his mind some associated idea. In the infant described by M. Taine the age at which ideas readily became associated seems to have been considerably later, unless indeed the earlier cases were overlooked. The facility with which associated ideas due to instruction and others spontaneously arising were acquired, seemed to me by far the most strongly marked of all the distinctions between the mind of an infant and that of the cleverest full-grown dog that I have ever known.

Curiosity, as M. Taine remarks, is displayed at an early age by infants, and is highly important in the development of their minds; but I made no special observation on his head. Imitation likewise comes into play. When our infant was only four months old I thought that he tried to imitate sounds; but I may have deceived myself, for I was not thoroughly convinced that he did so until he was ten months old. At the age of eleven and a half months he could readily imitate all sorts of actions, such as shaking his head and saying "Ah" to any dirty object, or by carefully and slowly putting his forefinger in the middle of the palm of his other hand, to the childish rhyme of

"Pat it and pat it and mark it with T." It was amusing to behold his pleased expression after successfully performing any such accomplishment.

I do not know whether it is worth mentioning, as showing something about the strength of memory in a young child, that this one when three years and twenty-three days old on being shown an engraving of his grandfather, whom he had not seen for exactly six months, instantly recognized him and mentioned a whole string of events which had occurred whilst visiting him, and which certainly had never been mentioned in the interval.

MORAL SENSE

The first sign of moral sense was noticed at the age of nearly thirteen months: I said "Doddy (his nickname) won't give poor papa a kiss—naughty Doddy." These words, without doubt, made him feel slightly uncomfortable; and at last when I had returned to my chair, he protruded his lips as a sign that he was ready to kiss me; and he then shook his hand in an angry manner until I came and received his kiss. Nearly the same little scene recurred in a few days, and the reconciliation seemed to give him so much satisfaction, that several times afterwards he pretended to be angry and slapped me, and then insisted on giving me a kiss. So that here we have a touch of the dramatic art, which is so strongly pronounced in most young children. About this time it became easy to work on his feelings and make him do whatever was wanted. When two years and three months old, he gave his last bit of gingerbread to his little sister, and then cried out with high self-approbation "Oh kind Doddy, kind Doddy." Two months later, he became extremely sensitive to ridicule, and was so suspicious that he often thought people who were laughing and talking together were laughing at him. A little later (two years and seven and a half months old) I met him coming out of the dining room with his eyes unnaturally bright, and an odd unnatural or affected manner, so that I went into the room to see who was there, and found that he had been taking pounded sugar, which he had been told not to do. As he had never been in any way punished, his odd manner certainly was not due to fear, and I suppose it was pleasurable excitement struggling with conscience. A fortnight afterwards, I met him coming out of the same room, and he was eyeing his pinafore which he had carefully rolled up; and again his manner was so odd that I determined to see what was within his pinafore, notwithstanding that he said there was nothing and repeatedly commanded me to "go away," and I found it stained with pickle-juice; so that here was carefully planned deceit. As this child was educated solely by working on his good feelings, he soon became as truthful, open, and tender, as anyone could desire.

UNCONSCIOUSNESS, SHYNESS

No one can have attended to very young children without being struck at the unabashed manner in which they fixedly stare without blinking their eyes at a new face; an old person can look in this manner only at an animal or inanimate object. This, I believe, is the result of young children not thinking in the least about themselves, and therefore not being in the least shy, though they are sometimes afraid of strangers. I saw the first symptom of shyness in my child when nearly two years and three months old: this was shown toward myself, after an absence of ten days from home, chiefly by his eyes being kept slightly averted from mine; but he soon came and sat on my knee and kissed me, and all trace of shyness disappeared.

MEANS OF COMMUNICATION

The noise of crying or rather of squalling, as no tears are shed for a long time, is of course uttered in an instinctive manner, but serves to show that there is suffering. After a time the sound differs according to the cause, such as hunger or pain. This was noticed when this infant was eleven weeks old, and I believe at an earlier age in another infant. Moreover, he appeared soon to learn to begin crying voluntarily, or to wrinkle his face in the manner proper to the occasion, so as to show that he wanted something. When forty-six days old, he first made little noises without any meaning to please himself, and these soon became varied. An incipient laugh was observed on the one hundred and thirteenth day, but much earlier in another infant. At this date I thought, as already remarked, that he began to try to imitate sounds, as he certainly did at a considerably later period. When five and a half months old, he uttered an articulate sound "*da*" but without any meaning attached to it. When a little over a year old, he used gestures to explain his wishes; to give a simple instance, he picked up a bit of paper and giving it to me pointed to the fire, as he had often seen and liked to see paper burnt. At exactly the age of a year, he made the great step of inventing a word for food, namely, *mum*, but what led him to it I did not discover. And now instead of beginning to cry when he was hungry, he used this word in a demonstrative manner or as a verb, implying "Give me food." This word therefore corresponds with *ham* as used by M. Taine's infant at the later age of fourteen months. But he also used *mum* as a substantive of wide signification; thus he called sugar *shu-mum*, and a little later after he had learned the word "black," he called liquorice *black-shu-mum*—black-sugar-food.

I was particularly struck with the fact that when asking for food by the word *mum* he gave to it (I will copy the words written down at the time) "a most strongly marked interrogatory sound at the end." He also gave to "Ah," which he chiefly used at first when recognizing any person or his own image in a mirror, an exclamatory sound, such as we employ when surprised. I remark in my notes that the use of these intonations seemed to have arisen instinctively, and I regret that more observations were not made on this subject. I record, however, in my notes that at a rather later period, when between eighteen and twenty-one months old, he modulated his voice in refusing peremptorily to do anything by a defiant whine, so as to express "That I won't"; and again his humph of assent expressed "Yes, to be sure." M. Taine also insists strongly on the highly expressive tones of the sounds made by his infant before she had learned to speak.

Finally, the wants of an infant are at first made intelligible by instinctive cries, which after a time are modified in part unconsciously, and in part, as I believe, voluntarily as a means of communication—by the unconscious expression of the features—by gestures and in a marked manner by different intonations—lastly by words of a general nature invented by himself, then of a more precise nature imitated from those which he hears; and these latter are acquired at a wonderfully quick rate. An infant understands to a certain extent, and as I believe at a very early period, the meaning or feelings of those who tend him, by the expression of their features. There can hardly be a doubt about this with respect to smiling; and it seemed to me that the infant whose biography I have here given understood a compassionate expression at a little over five months old. When six months and eleven days old he certainly showed sympathy with his nurse on her pretending to cry. When pleased after performing some new accomplishment, being then almost a year old, he evidently studied the expression of those around him. It was probably due to differences of expression and not merely of the form of the features that certain faces clearly pleased him much more than others, even at so early an age as a little over six months. Before he was a year old, he understood intonations and gestures, as well as several words and short sentences. He understood one word, namely, his nurse's name, exactly five months before he invented his first word *mum;* and this is what might have been expected, as we know that the lower animals easily learn to understand spoken words.

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Two—The Motor Development of Four Infants

MILLICENT W. SHINN

ABRIDGED FROM *Notes on the Development of a Child,* UNIVERSITY OF CALIFORNIA PUBLICATIONS IN EDUCATION, BERKELEY: UNIVERSITY OF CALIFORNIA PRESS, 1893-99, pp. 410-417, BY PERMISSION OF THE UNIVERSITY OF CALIFORNIA PRESS.

One of the most complete records of a young child's development was the record of her niece kept by Shinn. In addition to compiling a very full biographical account, she compared her data with other data that were available to her. Some of her comparative findings are presented below.

After the publication of her extensive notes, Shinn prepared from them THE BIOGRAPHY OF A BABY, *which is one of the best nontechnical descriptions of a child's development during the first year.*

The diagrams in Figure 1 summarize the chronological succession of the movements very briefly, and compare my results with those of other observers who have recorded at all fully the *process of acquiring* these powers. Most observers give only the *date of acquirement* of each movement recorded by them.*

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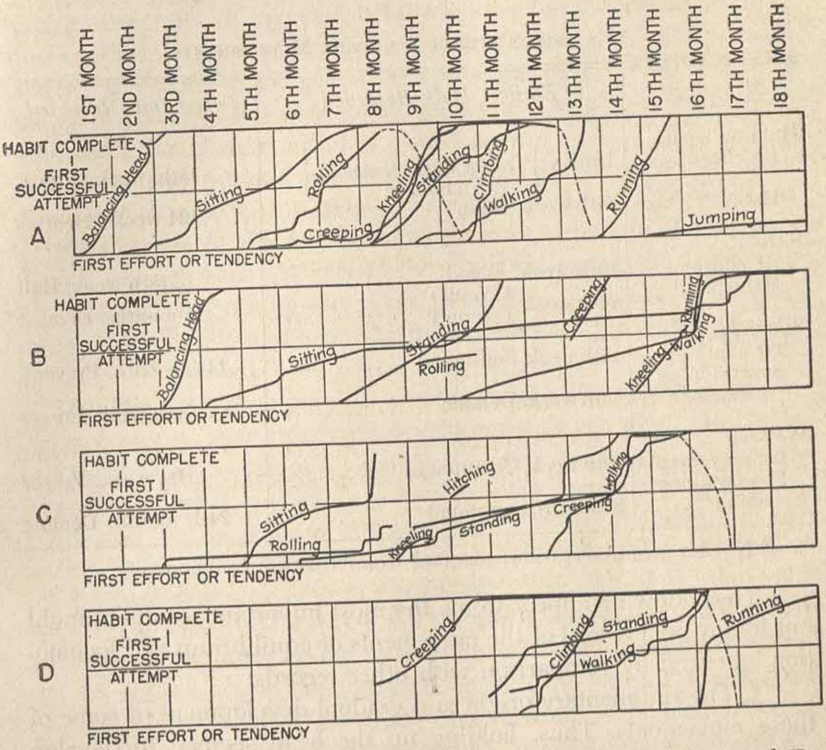
* Such bare dates are of little use even for mere comparison of the periods of completion in development, because in the gradual process each observer may set down a different stage as marking practical completion. He records, for instance, "Sat alone at six months old." Does this mean that the child for the first time succeeded in balancing for a second or two, or that sitting with secure balance was acquired as a habit for life? In the case of my niece, three months intervened between these two stages. Did the child sit on a cushion, lap, or other yielding surface, affording even the slightest support about the buttocks, or on a flat, hard surface? My record shows two weeks between these stages, and Mrs. Wood's a month, while Preyer also noticed that sitting on the hard level was a more difficult and later achievement. Does "standing alone" refer to the first successful balancing, or to secure equilibrium for minutes at a time? After the former stage was attained it took Mrs. Hall's boy two months to reach

I have tried to supplement these diagrams with a comparative table of such dates as are afforded by the reports of other observers: but so many of these are unavailable for comparison, that I abandoned the effort after any complete comparison, and contented myself with a mere table of extreme dates (Table 1), which will give an idea of the range of variation so far recorded. It should be noted, however, that in several cases earlier dates are reported than are given in the table, but not in such terms that I can be certain of



A. *Miss Shinn's record.* B. *Professor Preyer's record.* C. *Mrs. Hall's record.* D. *Mrs. Beatty's record. The dotted lines indicate the decline and disappearance of the movement. The broken lines show that the record was incomplete. In a few cases, however, as "rolling" in Professor Preyer's record, the movement itself was but partly developed by the child.*

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the latter; my niece required three months, and Preyer's child not less than *eight* months, the whole process of learning to creep, walk, and run coming in between. Even the order of succession of the movements is valueless for comparative purposes when we do not know what stage of the process of acquirement the note refers to; this is evident by a glance at the diagrams, with reference to the question whether creeping came before or after kneeling, for instance, in my record, or standing in Preyer's and walking in Mrs. Hall's.

the stage of development really meant: thus, for example, two of the Talbot papers report children as "sitting alone" at five months old, but I cannot tell whether this means that secure sitting alone for minutes together, on a perfectly level surface, was really attained.

TABLE I

EXTREME DATES OF ACQUIRING MOVEMENTS

Movement	*Earliest Date Recorded*	*Latest Date Recorded*
Holding head		
1st effort	1st day { Wood Talbot Papers, case E, two children }	11th week, Preyer
Habit	11th week, Shinn	24 weeks, Demme
Sitting		
1st effort	10th week, Wood	18th week, Hall
Habit	26th week { Wood Shinn }	11 months, Demme
Standing		
1st effort	18th week, Sigismund	14th month, Preyer †
Several minutes alone	40th week, Demme	70th week, Preyer
Walking		
1st movements	19th week, Champneys	41st week, Preyer
Free walking alone	8 months, Sigismund	24th month, Demme

† Not his own observation; reported from "trustworthy parents."

The following appear to me the most important results brought out by my own record of the movements of equilibrium and locomotion, checked by comparison with other records:

1. The rudimentary origin and gradual development of some of these movements. Thus, holding up the head seemed to develop out of a mere special innervation of the neck muscles, at times of heightened general stimulus; sitting began with a mere stiffening of the back against a support; creeping defined itself out of a medley of experiments in moving the body and limbs about. In some cases the steps of development seemed largely empiric, in others instinctive, and in some cases (notably in that of walking), the first sign of the movement seemed completely instinctive: but on the whole, careful watching seemed to reveal a more gradual process from smaller beginnings—to analyze the movements down into simpler elements, so to speak—than one would suppose from casual observation.

I do not find that any other observer reports quite so primitive beginnings in these movements as I do; but I suspect this is rather because the observer has not cared to push his record so far back than because such primitive stages did not occur.

2. The marked priority of the movements of the head, trunk, and arms over those in which the legs took part. There were two distinct periods—one (extending through the first seven months) in which the main development was in sense perception, together with the closely correlated power of grasping, but in which balancing and turning the head, sitting, and rolling, followed in distinct succession; and another (extending through a half year more) in which the main activity was in the development of locomotion and of equilibrium in the erect position, the movements developing alongside each other with much less distinct order of succession.

These two periods are clearly evident in the notes of other observers, though their duration varies. In all the records we have, the movements of the first period follow in strikingly similar succession, though Mrs. Hall omits record of balancing the head, and Preyer's child apparently was never given the opportunity to develop rolling. Mrs. Beatty and Mrs. Sharp do not record the earlier periods at all.

3. The late and empiric development of creeping. All the observers agree as to this tardy development of creeping. Moreover, it is the most uncertain and variable of all the locomotor movements. The use of hands and knees, which is more common than that of hands and feet, is perhaps easily accounted for by the length of the human leg; but this is the least of the variations. My niece drew herself forward with her elbows, flat on her stomach, before she could creep; Mrs. Sharp's eldest boy did the same; Preyer says of the first creeping, "Can creep, or rather drag himself along somewhat." Mrs. Wood's elder boy propelled himself "in measure-worm fashion," and her younger one wriggled along, and later drew himself forward with his arms, and at one time learned to steer himself in rolling, and used that as a means of voluntary locomotion instead of creeping; Mrs. Hall's boy hitched along in a sitting position before he could creep. Every one has known of babies who with every opportunity never creep at all, hitching or rolling instead; and Dr. Le Conte tells me of instances in which the creeping is tripedal, one leg being trailed along idle. It is curious to see so often reported an involuntary *backward* movement (highly displeasing to the child, whom it takes directly away from the objects he is trying to reach), immediately preceding successful creeping: so in my record, Mrs. Beatty's, and Mrs. Hall's.

4. The early development of climbing—in advance of walking —and also the strength of the climbing impulse. Mrs. Beatty's record corroborates mine strongly in this. Mrs. Sharp mentions climbing before standing and walking alone and so also does Mrs. Wood, if

we are to understand the "creeping upstairs" in the tenth month as properly climbing. Mrs. Hall does not record this movement at all, and Preyer alone places it late, in the third year. There are many indications that Professor Preyer's child was given far less opportunity for spontaneous muscular activity than the American children, and climbing is a movement quite dependent upon free opportunity; so that I think the other records on the whole confirm mine as to its early importance. Mrs. Chapman writes me that her three children all climbed before they could walk, and that a neighbor's baby, eleven months old, is now climbing, though unable to walk; and several other cases are reported to me.

5. The remarkable joy, pride, and physical exhilaration attending the acquirement of some of these movements. This appears more or less in all records, though in several cases the pleasure was marred by timidity, which my niece never experienced to a painful extent.

6. The similarity, on the whole, in the manner of development of the movements in different children. On casual comparison the variations are most striking; but on close examination of the records the only important difference seems to be that a movement is sometimes held back a long time, either by lack of opportunity or lack of confidence, and is then executed suddenly. For the rest, certain details of the process of development recur in the different records to a noticeable extent: as the wriggling or pulling forward before true creeping, and the involuntary backward creeping before the forward movement is mastered; the pulling to the feet, first by some low object so that the body is still partly propped by the arms, then by a higher, and later yet the rising from all fours.

Three—An Observational Study of the First Year

CHARLOTTE BÜHLER

ABRIDGED FROM CHAPTERS I AND X OF *The First Year of Life.* NEW YORK: JOHN DAY CO., INC., 1930, BY PERMISSION OF THE AUTHOR AND THE PUBLISHER.

No investigator besides Bühler has recorded for 24 hours each the behavior of more than 60 children, nor is such an undertaking soon likely to be repeated. For this reason, the material which she presents in THE FIRST YEAR OF LIFE *is very valuable. One would like to have more cases at each age level, but to attain this desire would involve monumental labor. Although at each of several age levels, Bühler reports on only five children, age differences in behavior are so great in the first year that large numbers are not necessary to portray developmental trends.*

The intention underlying our observations was to obtain a complete picture of the child's behavior during the first year of life. But further, we wished, by means of a very exhaustive inventory of every item of observable behavior, to obtain a characteristic inventory that would serve as a standard for average and normal development within this period.

METHOD

The method employed was that of uninterrupted systematic observation of one and the same child under those conditions which were normal in his everyday life. The child under observation was watched, whether waking or sleeping, over a period of 24 hours by observers who relieved each other at 8-hour intervals. The periods of observation were begun at different times of the day with the different subjects. The observer kept himself completely passive in relation to the child. He sat at the side of the child and in no way disturbed the normal daily plan. He at no time left the child, even when the child slept. And he kept accurate record of all that he observed. As often as a new behavior occurred, the precise time of its occurrence was noted. Where unusual precision was necessary because of the variability of the periods, a stop watch was used.

Even at night normal and customary conditions prevailed. The children lay in a darkened room and only a very faint light fell on them so that one could continue to perceive even the slightest change in conduct. This was the normal light for those children who were in the *Kinderübernahmsstelle*[*]—they were the largest percentage of the children observed in this investigation. The observer tried to avoid any disturbance of the child's sleep and, during the night, was, as a rule, separated from the child by the glass wall of the room in which the child lived. Whenever light was required for writing, the observer used a small electrical flashlight which he was careful not to reflect on the child. Each perceptible change in the childish behavior was noted and recorded so far as the means of descriptive language was able to represent them. It was found that we came very soon to the limit of language as a means of representation for impulsive moments and for vocalization, and in our later research we have made use of a finer method of observation and representation.

Since the observer did not permit the introduction of any artificial stimulus, there is lacking in our inventory whole groups of reactions which could have been invoked by the experimental method.

In order to avoid the possible errors that might have occurred had the observations been carried on by a larger number of workers, the greatest part of these protocols were taken by the same two observers. These had previously agreed, in complete detail, upon the method and the terminology.

SUBJECTS

In all, 69 children of the first year of life were observed, there being always 5 children of each month. Of these, 40 per cent were children from private homes and 60 per cent were "institution" children. Table I gives the age, sex, and number of the children observed.

By "children of private homes" we mean first of all those children who were observed in their home environment, and secondly those children who came to the *Kinderübernahmsstelle* with their mothers.

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[*] Reception House for Children in Vienna.

The remaining children under observation were largely children who came out of the family care to spend a three-week quarantine period in the *Kinderübernahmsstelle* before being placed either with a foster-mother or in some other institution. They were, therefore, in no way typical institutional children. Moreover, since these children came from a very varied social milieu, there was no need to fear that the material was in any way one-sided. A few errors in observation, illness on the part of the children, and similar conditions decreased the number of records. With the exception of the first group (the newborn) there were always five complete observations made in each age group. Since the children were not always the desired age precisely to the day, an allowance for a range of 14 days was made. Accordingly, a child either a week older or younger than one month was reckoned as one month. There was only one exception made, and that was with a group of newborn babies. This group consisted of four children from one to ten days of age, with an average age of five days (0–0 [+5]).[*]

TABLE I

AGE, SEX AND NUMBER OF THE OBSERVED CHILDREN

Age	0;0	0;1	0;2	0;3	0;4	0;5	0;6	0;7	0;8	0;9	0;10	0;11	1;0	Total
Child No.	*B1 B2 B3 B4	B5 B6 B7 B8 B9 B2 B10	G11 G12 G13 G14 G15	G16 B12 G17 G23 G24 G25	B20 G21 G22 B23 G24 G25	B26 G27 G28 G29 G22	G30 G31 G32 B33 G22	G34 B35 G36 B37 B38	G39 B40 B41 B42 B48 G30	B44 B45 B46 B47 G48	G49 B50 G51 B52 B53	G54 G55 G39 B60 B63	B56 G57 B58 B59 B61	
Boys	4	7	—	1	2	1	1	3	4	3	3	2	4	35
Girls	—	—	5	5	4	4	4	2	2	2	2	3	1	34
Total	4	7	5	6	6	5	5	5	6	5	5	5	5	69

*B = boy. G = girl.

The observations were carried out in the months of January to April, 1926. It is to be noticed that several times the same children were observed at different age levels so that the monthly progress in development could be seen in the case of one and the same child. The time spent on actual observation amounts, in round numbers, to about 1,620 hours. The protocols themselves fill 922 closely written pages.

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[*] The device for representing the child age O–O(+5) reads: no years, no months and five days. This system will be used throughout this selection.

RESULTS

From the point of view of our main problem, it is possible to treat the assembled material in three directions. First, the *qualitative* and *quantitative* analysis of the behavior; second, the time analysis of the day and the time measurement of the behavior; and third, *the meaning of the facts*—the establishment of the levels of development in the course of the first year of life. The qualitative and quantitative analysis of the behavior describes and determines the separate behaviors at the different age levels. Such as analysis establishes how the various ways of behavior modify themselves during the first year of life; it makes clear which behaviors disappear and substitute each other, which new one enters, and so forth. Such an analysis finally counts the many-sidedness of the series of reactions which are to be found in the different individuals at different age levels. The second problem is concerned with those ways of behavior which can be observed within the limit of one day, and with separating and determining those groups of reactions which follow one

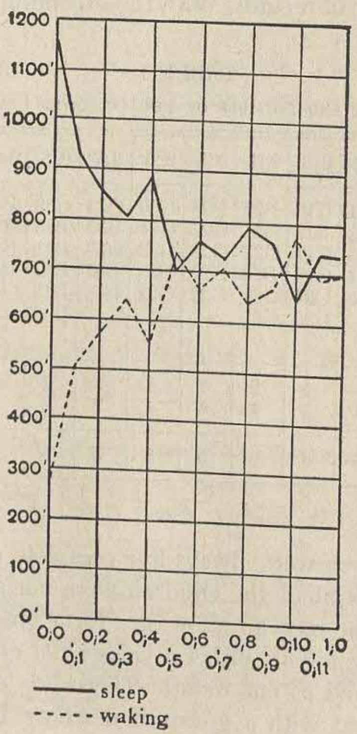


Figure 1. The day value for sleep and waking in minutes.

upon the other. The second problem includes also a consideration of the daily time value to be apportioned to the different groups of behaviors found in each age level. By daily value is meant the duration (in time) of a definite group of behavior observed within the period of 24 hours. The day values of different age levels can be compared with one another, and many and important items of value can be studied and collected.

From all these facts, obtained through the twofold analysis, it was possible to differentiate the development in definite periods and to establish the levels of development in the first year of life.

First of all we will have to separate the behavior during the sleeping state from that of the waking state. The numerical results of this separation can be seen in Figure 1.

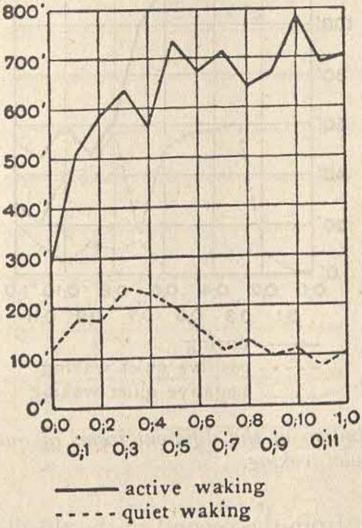


Figure 2. The relation between active and quiet waking periods.

We consider the decrease of the duration of sleep and the increase of the waking state to be the basic condition for any growing development, for it means a broadening of the time in which strength can be developed. The greatest decline of the sleeping curve is noticeable during the first quarter of the year. Twenty-one per cent of the daily value for sleep of the newborn is lost in that time. From the third month to the end of the first year, the curve for sleep loses only 7 per cent more of its daily value. The fifth month is decisive in that the sleeping and the waking states are evenly divided over the day.

The waking state can be either quiet or occupied by some sort of movement. The relative importance of these two states at each age level is shown in Figure 2.

The day value for the dozing state, which in the newborn takes up 100 per cent of the quiet waking state, fills only 53 per cent of that of the one-year-old. We can observe the decisive change, the sudden decrease of the dozing state, in the eighth month. This is shown in Figure 3.

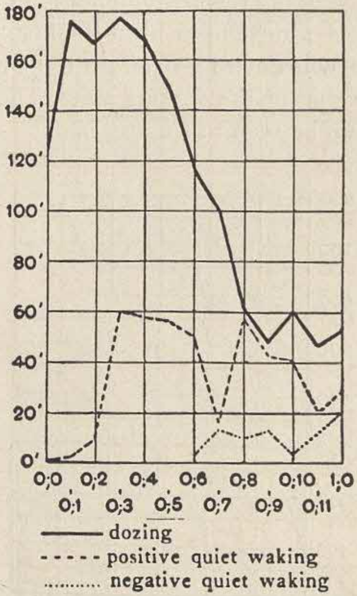


Figure 3. The distribution of the different forms of quiet waking in relation to the day value of quiet waking.

We notice that from the second to the third month that part of the day value for quiet waking which is taken by the receptive waking state now increases. With the appearance of contemplative waking, the part of the receptive and contemplative waking state increases percentually once again, only to lose in significance once more during the last quarter of the first year. This can be explained by the fact that the child now reacts with active interest to all sense perceptions and no longer meets them in a quietly receptive state. The decrease of the dozing state can be traced through the entire first year. While the quiet waking state of the newborn was filled up entirely by dozing, from the eighth month on only half of that state is thus occupied. The state of quiet displeasure, which increases, fills about one-quarter of the quiet waking of the one-year-old. It increases because the state of restless displeasure decreases. From among the forms of quiet waking we join together the state of dozing and sleep, putting them into a common group. Both decline considerably during the first year of life. In the same degree in which sleep and unoccupied waking decrease, the waking state, which is filled with impressions and actions, increases. The latter is usually a state of movement, seldom one of rest. In connection with our qualitative analysis we tried to calculate the day value for the three different groups of reactions—for the negative, positive, and spontaneous reaction. The value thus estimated is shown in Figure 4.

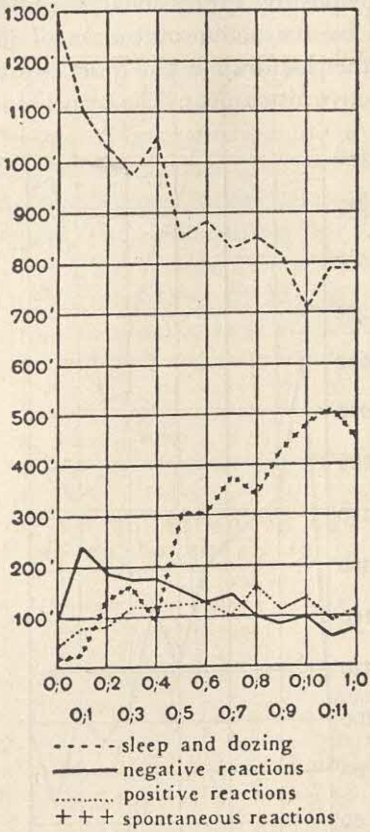


Figure 4.

Comparing these results we find decrease of day value for sleep and dozing during the first year of life. Decisive changes take place between the fifth and tenth months. After reaching a maximum in the first month there is a decrease of negative reactions, and an increase of positive reactions from the first day of life. The greatest day value increase, however, is found in the part given to spontaneous reactions. In these, the fifth and ninth months reveal an especially great development.

The day value for positive movements of expression grows during the second half year of life, when some active expressions of joy can be observed in addition to a quiet waking state of comfort. We have observed that this quiet state of comfort is a condition first accompanied by positive expressional movements and only much later accompanied by the active outbreaks of joy, while negative reactions, on the other hand, show the quiet expression of ill humor much later than active utterances. The appearance of these active

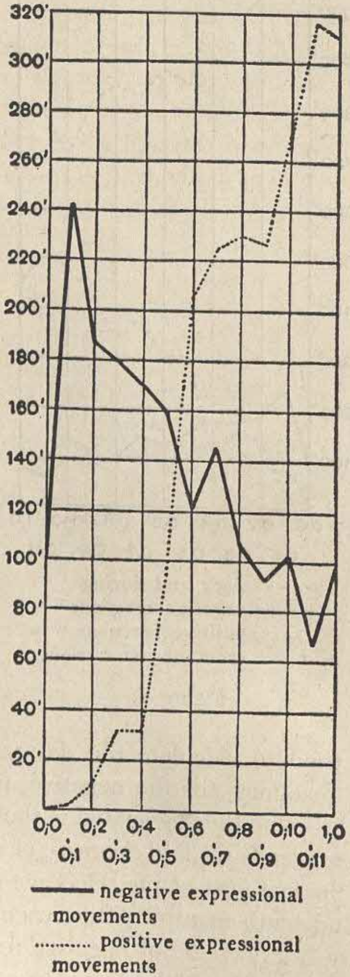
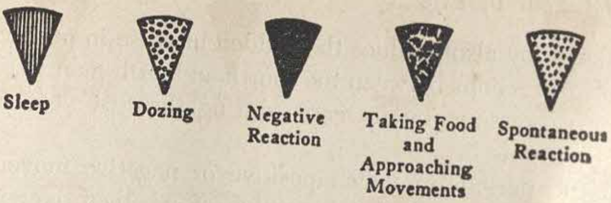


Figure 5.

outbreaks of joy also produce the sudden increase in positive movements of expressions between the fourth and sixth months. That the behavior expressing joy increases during the first year is shown clearly by Figure 5.

All the periods in which positive or negative movements of expression were observed, irrespective of whether they were the dominating factor or just different ways of behavior (experimenting, single movements, reception of food), are here grouped together according to time and contrasted with each other. We can state concerning them that positive expressions appear much later than negative ones, and that, although at first they are far less than the negative, they finally exceed them considerably during the first year.

How the duration of the various groups of behavior is distributed during the day can be seen in the following typical diagrams which we call day cycles. The circle corresponds to the 1,440 minutes of the day; the values of the various groups are entered as sectors. In these diagrams (Figures 6 through 12) we can study the forward steps in development as easily as in graphs and tables.



Key to Figures 6 and 7.

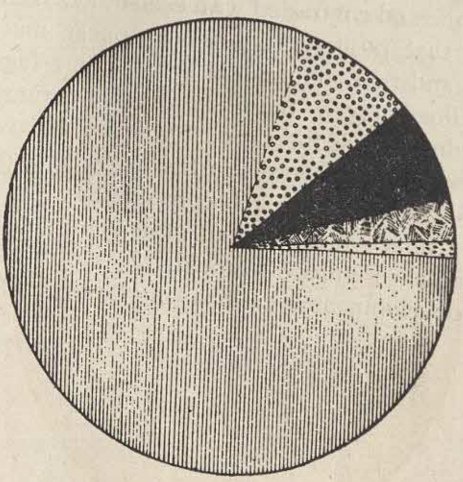


Figure 6. Typical day cycle at birth.

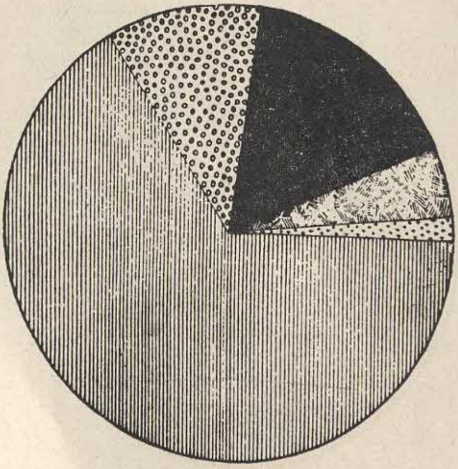
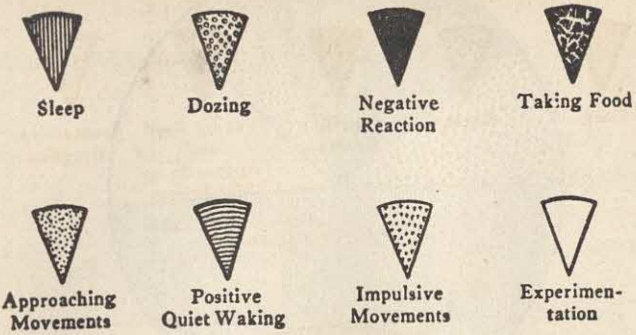


Figure 7. Typical day cycle at one month.



Key to Figures 8, 9, 10, 11, and 12.

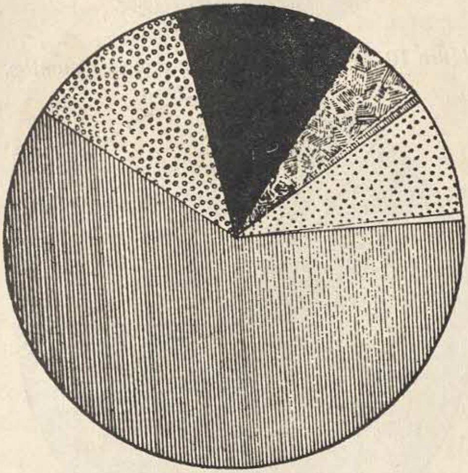


Figure 8. Typical day cycle, age two months.

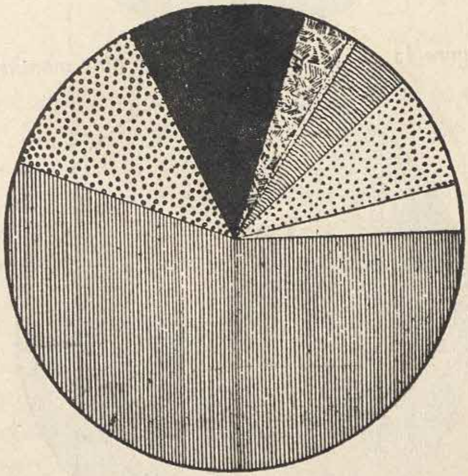


Figure 9. Typical day cycle, age three months.

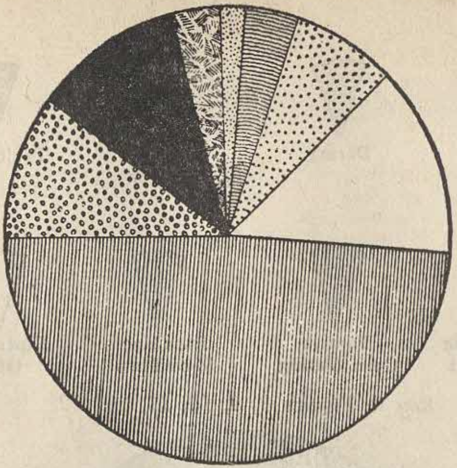


Figure 10. Typical day cycle, age five months.

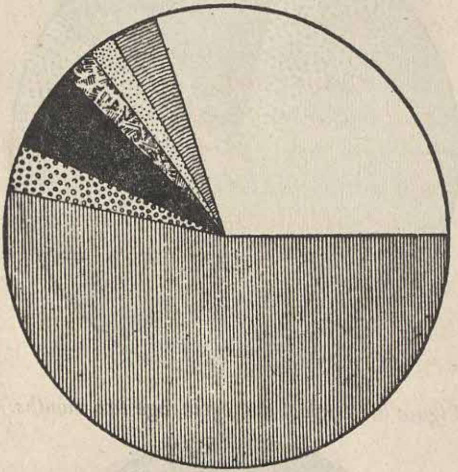


Figure 11. Typical day cycle, age nine months.

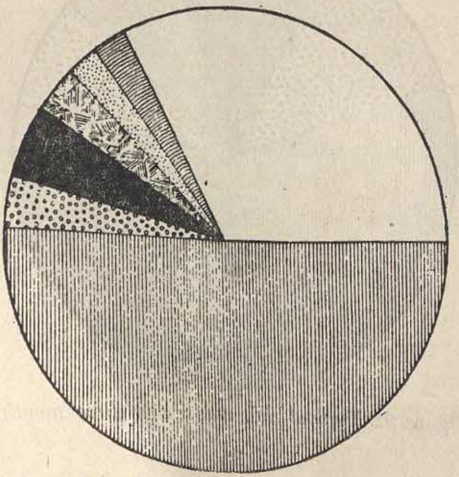


Figure 12. Typical day cycle, age one year.

Four—A Longitudinal Study of the Motor Sequence

MARY M. SHIRLEY

REPRINTED FROM PAGES 4-8, 34-46, OF VOLUME 2 OF *The First Two Years.* MINNEAPOLIS: UNIVERSITY OF MINNESOTA PRESS, 1933, BY PERMISSION OF THE PUBLISHER.

Information concerning when a given response BEGINS *or* ENDS *can come only from continuous or repeated observations of the same individual. Shirley's study, conducted at the Institute of Child Welfare at the University of Minnesota, was the first investigation to apply uniform methods of recording to a sizable group of children over a period of two years. The findings presented here represent only a small part of her results. This longitudinal approach is now being used at many child research centers. In some instances, data on the same individuals from birth to maturity are now available.*

METHODS

Whatever contributions this study may make to our knowledge of infancy will be largely attributable to the method used. Certain fundamental problems of development have only one avenue of approach, that of long continued, or frequently repeated, observations of the same individual. A view of the child as a whole and an understanding of the detailed course of child development, its continuity and its rate, are the rewards of pursuing this route.

Thus far the longitudinal road has been sparingly traveled by students of child development, the reason being, of course, that one can progress much faster and much further toward an understanding of many fundamental principles by taking a short cut that crosses the lives of many children at the same time than by following a few babies down the long lanes of the months and years. For many years interested parents and relatives were the only ones to undertake such long journeys, the obvious reason being that they were perhaps the only persons who had continuous and easy access to the child over long periods. With all their obvious faults and defects, the contributions of these biographies of children kept by parents and relatives are of inestimable value to the modern student of child development. From them one not only gets an impression of the continuous onsweep of developmental processes, but he also feels intimately acquainted with the child as a person.

The growth of the intelligence-testing movement and the theoretical problem of the constancy of the IQ that it has raised has resulted in the practice of making "follow-up" studies, whereby individuals tested or measured at an early age are examined again at a later age. Perhaps the largest and most completely organized follow-up work has been done by Terman and his co-authors in their *Genetic Studies of Genius*. Indeed, so comprehensive is the study and so continuous is the contact that it might be called the first of the "follow-through" studies, if we may designate as follow-through studies those in which the contact is maintained over a long period and observations are made at repeated intervals. These are in contrast to the "follow-up" studies, in which the contact may be broken and re-established once or oftener during the individual's childhood. In the field of follow-up work Gesell has retested enough children to enable him to draw up developmental curves.

The present study is, so far as the author is aware, the first of the follow-through studies of babyhood. A few months after it was begun, a similar study was initiated at the University of California, where Bayley and Wolff made repeated monthly examinations of a group of 60 babies. The Minnesota study does not include so many subjects, but the deficiency in that respect is perhaps offset by the greater frequency of the examinations and by the fact that the study was conducted in the homes instead of being confined to the laboratory.

In brief, the general procedure consisted of observations of a sizable group of babies in the hospital and in the homes from the day of birth. Examinations were made daily during the first week of the hospital period and every two days during the second week. During the remainder of the first year the babies were observed at weekly intervals in their homes and at biweekly intervals during the second year. At the age of two years two of the children became regular attendants at the Institute nursery school and entered into the regular program of observations and tests carried out by Institute workers. The remaining 17 children, whose parents still live in the Twin Cities, are visited four times a year for the collection of anthropometric data and twice for psychological data, and once a year they are brought to the Institute for a mental test. The weekly examinations of the first year were supplemented by the mothers' daily records of food intake and habits of sleep and by their summaries of elimination, developmental items, and general behavior.

By virtue of its follow-through character this study for the most part treats the order of development without respect to age. Conducting the observations in the home introduced considerable informality into the examining situation, and the systematic record that was kept of incidental reactions in addition to responses to the tests greatly increases the value of the data as an index to the ordinary course of behavior development. In view of the similarity of this approach to that of the biographical studies, it is not surprising that there are similarities in the results. The course of development, as will appear later, is so consistent from baby to baby that it may be studied more effectively by the careful, continuous observation of one baby than by a cross-section survey of many babies at widely spaced age intervals.

Similarity of the two methods may account likewise for the high consistency of this study with that of Bühler. Her method was to observe constantly for an entire day and night the activities of a small group of babies on each monthly birthday. The observations were made without artificial stimulation; the observer merely sat near the infant, watching and recording his spontaneous behavior in his usual daily environment. The present study differs from Bühler's in that simple tests were planned in the hope of tapping the baby's entire repertoire of acts in a brief half hour of examination each week. However, the examining situation soon became a usual part of the baby's routine, and the mother's presence and the familiar home environment completely robbed the examination of all strangeness. The tests themselves closely duplicated the parents' play stimulation and were appropriate for calling forth the babies' abilities and interests. Responses to the tests were recorded in descriptive terms rather than in terms of arbitrary standards of behavior, and the records of incidental behavior and the mothers' records adequately supplemented the test data. For all these reasons it is likely that this material is almost as free from the unfavorable influences of artificial stimulation as is that of Bühler. It presents the additional advantages, moreover, of being somewhat more controlled and of including a larger number of babies of each age than Bühler's. The qualitative descriptions of infant behavior obtained by the two methods are strikingly similar.

Before we attempt to say just what the present study has added to our knowledge of motor development let us consider briefly the things it has not added. Only with a clear understanding of the scope of a piece of scientific work may its contributions be evaluated and allocated among the existing body of facts.

There are four or more courses that a student of motor development in infancy might follow. He might set up age norms of development by means of a study of large numbers of babies; he might measure the degree of motor skill possessed by the individual baby; he might devote his attention to the modification of given motor items after they were established and determine the conditions that led to modification; or he might interest himself in the sequence of motor development. In this study the fourth course was followed; a few minor facts relating to the other three were gathered, but they were only incidental to the main objective.

The reason for rejecting the first three methods may be briefly stated. In the first place, age standards of development derived from so small and so selected a group of babies as entered into this study would be valueless for future comparisons. It cannot be stressed too often that the age figures given in this work apply to this group alone and are not to be used as criteria for other groups. Far better norms have already been established by Gesell, Jones, and others, and therefore there is no great need for a normative study at the present time.

A study of the degree of motor skill possessed by each baby could be conducted only if accurate ways of measuring the skill could be devised. This is not beyond the limits of possibility, but it is not an easy task. The difficulty of even estimating degrees of head control and the skill involved in sitting alone and other motor accomplishments is baffling. Although the investigator is able to judge the degree of skill in an older child by putting him through a strict set of performance tests, it is likely that he would have to limit the items in his tests of babies to the things they do spontaneously in order to insure their wholehearted cooperation. Such a study would entail the establishment of standards of degree of skill. Worth while as the investigation would be, it could be carried out only with the aid of better controlled experimental conditions and more refined examining techniques than were possible in this investigation.

Likewise, a study of the nature and degree of modification that motor skills undergo from their earliest manifestation to their perfection would require more time and more elaborate treatment than was possible in this study. One phase of motor development, walking, has been fairly adequately studied in this way, but techniques for similar studies of other aspects of motor development were not available, so they were not attempted.

There remains the developmental order, or motor sequence, the only aspect of motor development on which this study offers new and reasonably complete data. Before entering upon a description of the sequence and a discussion of its importance it is necessary to make clear two distinctions: first, the difference between motor activity and motor control, and, secondly, the difference between ability and proficiency in the performance of motor acts.

Motor activity versus motor control. In centering attention on the order of motor development the observer did not fail to take cognizance of the profuse and varied activity present in the babies at birth. It is difficult to define this activity in such a way that the differences between it and the controlled acts that developed later may be readily apparent. The early activities of kicking, waving, and squirming were not uncoordinated, nor were they altogether random and spontaneous. Perhaps the most characteristic earmark of the activity of this period was that it "didn't get the baby anywhere"; it apparently was directed toward no goal, or if it was it did not effect an adjustment to the environment. Controlled motor acts, on the other hand, were successful responses to definite stimuli, and with the development of motor control the baby began to manipulate and manage his environment to a considerable degree. The motor sequence represents only the development of motor control and not the manifestation of undirected motor activity.

Ability versus proficiency. The reader must also remember that there is a great difference between ability to do and excellency in the doing of motor acts. The motor sequence takes into account only the former. It represents the order in which the babies were first able to watch, to reach, to sit alone, and so forth; it is not concerned with the proficiency with which they performed each act. It goes without saying that the babies differed in proficiency at the onset of each new phase of motor development even though the degree of difference could not be measured, and that proficiency increased in individual babies at different rates and to different degrees. But the transition from the inability to reach, creep, or walk to the ability, however feeble, to perform these acts is well marked. The order of these transitions from uncontrolled generalized activity to successfully controlled motor acts comprises the motor sequence.

Sequence not a by-product of the motor tests. The reader may question whether the motor sequence is not merely a product of the motor tests. That possibility was safeguarded against in two ways; first, by the recording method, by which the observer wrote down in consistent descriptive terms exactly what the baby did in response to each motor test instead of marking them passed or failed; and, secondly, by including in the sequence items that were not tested for but that occurred spontaneously. The consistency with which the items of motor play fit into the sequence is a strong argument against its artificiality.

SEQUENCE OF MOTOR DEVELOPMENT

Much has been made of the point that motor development in the first two years follows a definite sequence, at least when its various phases—postural control, locomotion, and manipulation—are considered separately. It remains to be seen whether the sequence holds when these different aspects of development are thrown together into one series. Under such treatment the sequence might conceivably break down; granting that the sequence comes about by virtue of the fact that coordinations acquired through the perfection of one motor item are essential components of the item next developed, then the sequence of these items would be a logical necessity. Undoubtedly this is true in such a sequence as that of reaching, touching, grasping, and retaining; it is mechanically impossible for the baby to grasp a toy without having first reached and touched it, although it is not quite clear why the ability to do these three acts does not appear simultaneously instead of at intervals of one or two weeks. It is a little less obvious but still well within reason that chest up succeeds chin up, rolling succeeds swimming movements, and creeping succeeds scooting backward, because coordinations acquired in preceding acts are used in the ones following. If this explanation fully accounts for the sequence within any given phase of development, such as manipulation or assumption of an upright posture, one might expect to find no sequence from one type of motor performance to another where the coordinations acquired in the first motor act seemingly play no part, or an obscure and minor one, in the second. One would not expect that head control would precede reaching in the lying posture, or that reaching, grasping, and holding would precede sitting alone, nor would the sequence of the play reactions of scratching, playing with the toes, bouncing, and rocking be an obvious inference.

When all the motor items are thrown together, however, the sequence is just as well defined as when each motor phase is considered separately. To be sure, some items of the manipulatory phase develop simultaneously with items of the locomotor and postural phases, but simultaneity of items does not impair the fundamental

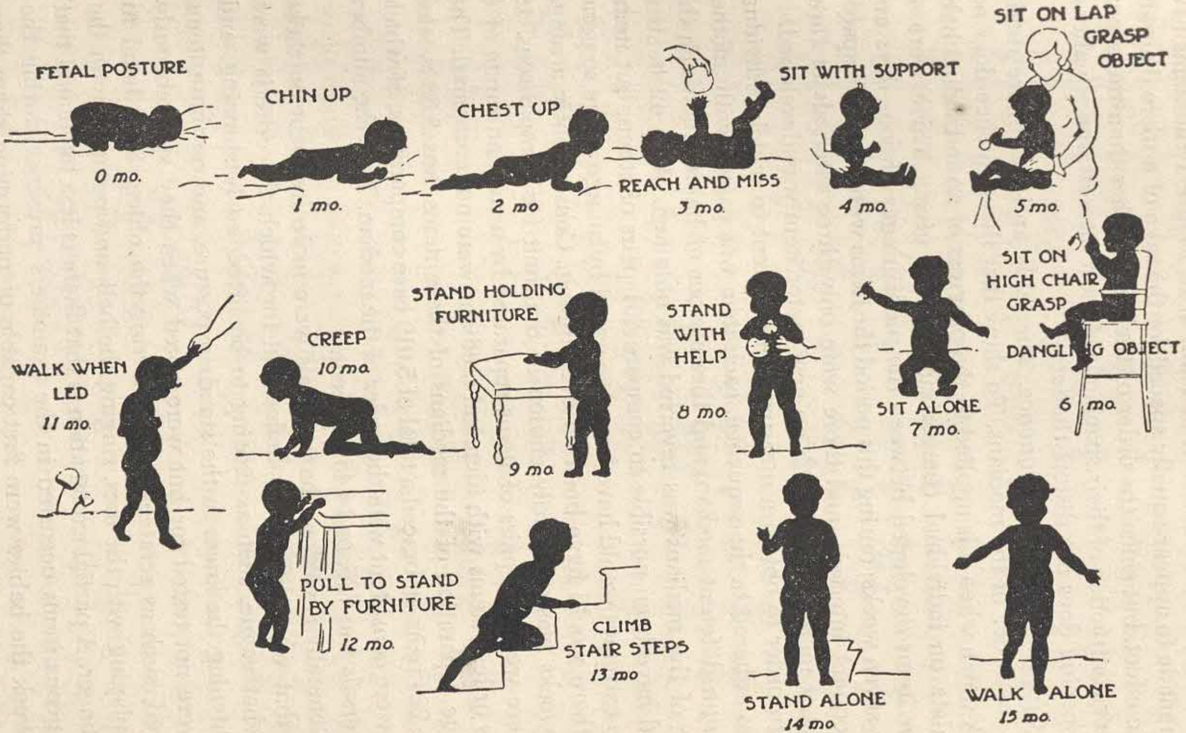


Figure 1. The motor sequence.

sequence. For the most part, the items of one phase dovetail with those of the others. The extent of simultaneity and of dovetailing in the sequence may be seen in Table I. In Figure 1 the items have been made to appear equally spaced for the sake of a more diagrammatic effect, but since the order of the items rather than the expansion or contraction of their spacing is of most interest, the equality of the spacing does not distort the facts.

Consistency of the sequence. The table and the figure depict only the order of the medians. To show that the sequence does not break down when it is applied to the progress of an individual baby the data on individual comparisons will be given. There were 42 motor items developed between the median ages of four days and forty-seven weeks; during this period the items were naturally spaced at frequent intervals, and there were only three intervals of three weeks or more during which no new motor item was developed.

In order to obtain a measure of the extent to which individual babies adhered to the sequence, each item was paired with another having a different median, and the number of babies in which the order of the medians was reversed was obtained. With 42 items it would have been possible to compare 861 pairs of items, but items at the extremes would have been separated by an age range so great that there would have been no overlapping. Consequently a range of 6 weeks was arbitrarily chosen as the limit for comparisons. In all there were 224 pairs of items separated by a median range of 6 weeks or less; items with identical medians were not compared. The average separation of the medians of these items was 3.66 weeks. These 224 pairs embraced a total of 5,019 case comparisons, of which 4,294 were consistent with the order of the median. Thus the number of reversals was 725, or 14.45 per cent.

Generally speaking, the reversals were more frequent on those items that were difficult to define and for which the criteria were somewhat vague, such as tensing to be lifted, early stepping, and straightening the knees in the standing posture, and on those items that were not tested for but were noted when they spontaneously occurred, such as scratching, transferring the object from hand to hand, playing with the toes, ringing the bell, and pointing with the index finger. A partial explanation is that there is less likelihood that spontaneous items occurred in the examiners' presence during the same week the babies were first capable of performing them than that the items tested for were called forth and observed as soon as the capacity developed. Moreover, examinations were occasionally missed; if several new motor items appeared simultaneously at the next examination the observer might fail to note all of them, and thus

TABLE I

MEDIAN AGES AND PARTS OF THE BODY CONTROLLED IN VARIOUS REACTIONS IN THE MOTOR SEQUENCE

Median Age in Weeks	*Eye Coordination*	*Locomotion*	*Fine Motor Reactions*	*Motor Play*	*Parts of Body Controlled*
6 da.	Follow light				Eyes, head, and neck
2.0	Watch person				
3.0		Chin up			
5.0	Follow tape H				
9.0	Follow tape V	Chest up		Smile at person	
10.0	Follow tape C				
14.0		Adjust for lifting	Reach and touch		Arm and upper trunk
15.0		Sit on lap	Grasp object	Play with hands	
18.0			Retain object (thumb opposition)		
21.0			Grasp dangling object	Object to mouth	Hands and lower trunk
25.0		Sit alone momentarily	Transfer object from hand to hand	Play with toes	
28.5		Roll		Pat toy	
30.5		Sit alone 1 minute		Rock, shake head	
31.5		Stand with help			Pelvic region, legs, and fingers
38.0		Some progress on stomach		Suspension bridge	
41.0		Scoot backward		Pat-a-cake	
42.0		Stand holding to furniture	Point with index finger	Peeka-boo	
45.0		Creep			
45.0		Walk when led			
47.0		Pull to stand by furniture	Open simple boxes		
56.0-62.0				Put fingers in holes	
62.0		Stand alone			
66.0		Walk alone		Run, climb	

the possibility of reversals would be increased. Considering all these possibilities for error, the wonder is not that reversals in the sequence are so many but that they are so few.

The sequence was further tested for consistency by running rank order correlations between the order of items for each baby and the order of the medians. These correlations which are given in Table II, all lie between .93 and .98, and 60 per cent of them are .97 or above. The consistency of the correlations adds much to their significance, since it shows conclusively that the sequence is as representative of individuals as it is of the group as a whole. From baby to baby the sequence holds with far greater consistency than could be accounted for by chance.

TABLE II

INDIVIDUAL CORRELATIONS WITH THE MOTOR SEQUENCE

Baby	*r*	*Baby*	*r*
Winifred	.95 ± .010	Martin	.93 ± .012
Fred	.98 ± .009	Quentin	.97 ± .009
Carol	.96 ± .010	Virginia Ruth	.96 ± .010
Doris	.94 ± .010	Sibyl	.93 ± .012
David	.95 ± .010	Maurice	.95 ± .010
Donovan	.97 ± .009	Torey	.98 ± .009
Harvey	.97 ± .009	Judy	.97 ± .009
James D.	.97 ± .009	Peter	.98 ± .009
Irene May	.97 ± .009	Patricia	.97 ± .009
Larry	.97 ± .009	Walley	.97 ± .009

Harmony with anatomical laws. The well-known anatomical law of developmental direction—which may be stated briefly as the law that the growth wave which sweeps over the body begins at the head and travels toward the feet—has already been mentioned as being exemplified by the assumption of postural control. From a study of Table I it would appear that not postural development alone but the entire motor sequence is in harmony with this fundamental law of growth. The eye muscles come under control first; a little later control of the facial muscles for smiling and of the neck muscles for head-lifting and head-turning is achieved. Gradually motor control creeps downward to the arm and upper trunk region; with the advent of sitting alone it has migrated down to the lower trunk; and when thumb opposition appears, it has advanced to the forearm and hand. Finally the leg muscles come under control, first for standing erect with help, somewhat later for locomotion in the creeping posture, and still later for walking alone.

Not only is the order of development in harmony with the law of developmental direction, but the speed of development also seems to be graduated from the head downward. Control of eyes, head, neck, upper trunk, and arms follow each other in rapid succession. About the time the ability to sit alone is achieved, however, the rate becomes noticeably slower. Whereas the attainment of a new motor item was a matter of days at the outset, it now becomes a matter of months. These differences in the rate of assuming motor control suggest that the growth gradient that makes for different rates of growth in different parts of the body is also operative, or at least has a counterpart in the development of functional control.

A second interesting feature of the sequence is that postural control of a given part always precedes controlled movements of that part. The baby holds his head erect before he turns it; he sits alone before he sways and rocks in this posture; and he stands before he climbs or walks. This is probably just a matter of body mechanics; it would be impossible to execute coordinated movements in a given posture before the posture itself could be maintained. The progress from flexion to extension in babyhood is, however, worthy of note. In the newborn baby the dominance of the flexor muscles over the extensors is so great that arms, hands, and legs can hardly be stretched out for measuring. Gradually the extensors begin to oppose this state of flexion, and the early flexed lying posture gives way to more extended postures. It is largely the extensor muscles that are involved in the maintenance of upright postures. Investigators are still pretty much in the dark as to the nervous mechanism of postural control, but whatever the mechanism, it is logical for extensor muscles to come under control for posture before they work together with the flexors to execute directed acts involving a rapid losing and regaining of postural equilibrium.

Maturation as an explanation of the motor sequence. In order to prevent misunderstandings that might lead to controversy, the term "maturation" must be rigidly defined. In this study it is used to connote the sum total of the growth processes. It is not used in the more restricted sense of the development or maturing of the nervous system alone.

Maturation, defined in this way, appears to be the most satisfactory way of accounting for the motor sequence. The number of items included in the sequence and the closeness of their spacing certainly precludes the possibility of a consistent order being established by chance; the consistency of the sequence from baby to baby, not only in the motor functions tested but also in spontaneous play reactions, casts doubt upon the learning hypothesis as an explanatory principle. Finally, the harmony of the sequence with the fundamental law of anatomical growth is a strong argument for the acceptance of the maturation hypothesis.

How maturation functions. In accepting the maturation hypothesis it is not necessary to settle the question as to whether the motor sequence is a function of growth or whether it is merely a manifestation of growth. It may be that the relationship between body growth and motor development is one of cause and effect, or it may be that the two are merely parallels, the one being the manifestation of growth in body structure and the other the manifestation of growth in body function. But in either case maturation is at work.

The present study does not enable us to say what matures or how maturation operates in bringing about the motor sequence. No doubt the fact that motor control begins at the head and moves toward the feet is partly a matter of body mechanics; to a certain extent it is true that outlying muscles are incapable of making adequate adjustment to peripheral stimuli until the larger, proximal muscles work to bring them directly in contact with the stimuli. It is also true that motor development consists largely of the differentiation of specific movements from generalized mass activity. Neither of these possibilities, however, weakens the sequence itself nor precludes the interpretation of it by the maturation hypothesis, since neither of them is adequate to completely account for a sequence of such remarkable length and detail. The consistency of the sequence from baby to baby speaks well for its reliability, and its harmony with the accepted laws of growth attests to its validity as a maturing process.

SUMMARY

Progress in motor control follows an orderly sequence beginning at the head and traveling toward the feet. The sequence as determined from the medians of the group holds for individual babies, and it includes, in addition to items specifically tested for, items of motor play that occurred spontaneously. Hence it is unlikely that the sequence is merely the by-product of the tests. The sequence of development strongly supports the maturation theory of motor development. It appears that maturation gives ability to do motor acts and that subsequent practice gives proficiency in doing them.

Five—Infant Reactions to Medical, Anthropometric and Psychological Examinations

MARY M. SHIRLEY

REPRINTED FROM PAGES 31-40 OF VOLUME 3 OF *The First Two Years,* MINNEAPOLIS: UNIVERSITY OF MINNESOTA PRESS, 1933, BY PERMISSION OF THE PUBLISHER.

In this section, additional results from Shirley's longitudinal study of 25 infants are presented. It will be seen that she was interested in social and emotional development as well as motor development. A further report on Shirley's babies at the age of seventeen is presented in Chapter VIII.

TEST SITUATIONS THAT PROVOKE IRRITABILITY

In order to determine whether some test situations provoked fussing more frequently than others, the amount of irritability was computed for certain test items on each type of examination. This method of scoring by items was somewhat complicated and will have to be explained in some detail. For the anthropometric examinations the different measurements were grouped in six general classes according to the part of the body measured and the type of manipulation involved. These groups were head measurements, trunk and arm circumferences, trunk bilateral diameters, leg and foot measurements, Babinski reflexes, and trunk anterior-posterior diameters. During the first five of these the baby usually lay on his back; during the last he lay on his side. The number of items at which screaming or fussing occurred was counted for each group of measures on each examination, and the score was converted to percentage by dividing by the number of items in each group.

In addition to these six general groups there were three specific measurements that seemed to cause fussing more often than others.

These were the sacro-pubic and the sterno-pubic measurements, which were taken with calipers, and the crown-rump and crown-heel lengths, which were taken with the baby lying on a special measuring board. In scoring these individual items for irritability it was necessary to take into consideration the baby's irritability just before that measurement was taken. If the baby fussed only at these measurements then the situation itself must have provoked the irritability. If, however, the baby who screamed at these measurements had been screaming all along, then there could be no way of knowing whether the situation itself provoked the irritability. Consequently screaming, fussing, or placidity at the particular measure was recorded by *S*, *F*, or *O;* and a subscript *s* or *f* was added to indicate that the baby had fussed or screamed at one-fourth of the items in the group of measurements just preceding this single measurement. Groups of measurements were not always taken in the same order; the examiner sometimes skipped from one group to another so as to interfere as little as possible with the child's activities. In general, however, the different measurements within a group were made in a regular order.

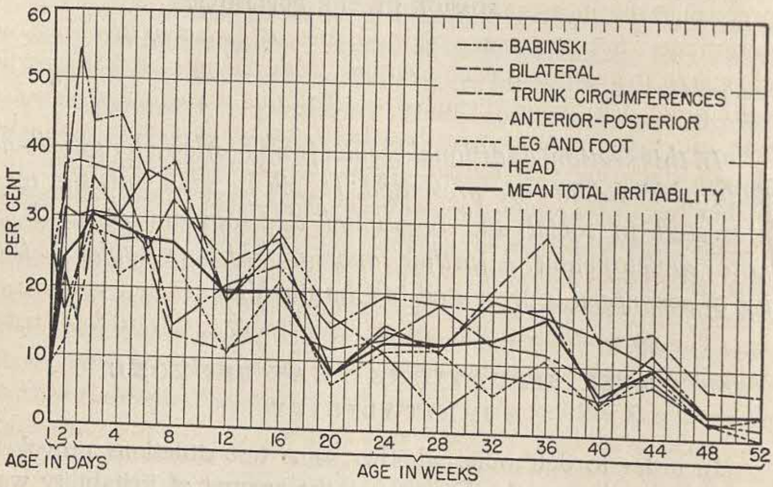


Figure 1. Irritability at several types of measurement throughout the first year. Percentage of total possible irritability score.

Irritability at groups of measures. The results of this treatment of the data are shown in Figure 1. In general the developmental curves representing each group of measures follow that for all the measures; this of course is to be expected. There is no constant tendency for one group of measures to arouse little irritability and another group to arouse a great deal. It is interesting to note that whereas leg and foot measurements elicit slightly less irritability than most groups, Babinski reflexes call forth slightly more during the first five months. Irritability during the head measurements is slightly lower than that of other groups in the first 12 weeks, slightly higher from 20 to 36 weeks, and about the same as the others during the rest of the year. In general, though, the differences between groups are too slight to be remarked upon. There is no tendency for a baby to be consistently irritable when a particular part of the body is measured.

Sacro-pubic, sterno-pubic, and length measurement. As has been mentioned, the data on irritability at these specific measures were tabulated so as to take into account the irritability of the baby just preceding the measurement. Further computations were made to determine what percentage of the babies at each age level had no change in irritability at each measure; to the number of babies who were quiet before and remained quiet during a particular measure was added the number who had been screaming before and who screamed during that measure, and this sum was converted into percentage of the whole. Similarly, the percentage of the babies who changed toward greater irritability was found by adding the numbers of those who had been quiet before but who fussed or screamed at the specific measure and those whose fussing increased to screaming. The percentage of the babies who changed toward less irritability was obtained in like manner (see Table I).

TABLE 1

PERCENTAGE OF BABIES SHOWING DIFFERENT AMOUNTS OF IRRITABILITY AT CERTAIN MEASUREMENTS DURING THE FIRST YEAR

Reaction	*Sacro-Pubic Measurement*	*Sterno-Pubic Measurement*	*Length Measurement*
No change in irritability	77.7	74.3	59.7
Greater irritability	16.5	16.8	34.1
Less irritability	5.8	7.6	5.7

During the first 8 weeks the babies showed a tendency toward increased irritability at the sacro-pubic measure. Although about 55 per cent remained unchanged at this measure, almost 35 per cent changed from less to more irritability and only about 10 per cent changed from more to less irritability. After 12 weeks, however, the sensitiveness to this measure suddenly diminished, and during the rest of the year about 90 per cent of the babies remained unchanged at this measure. The totals for the whole year as given in Table I lie between the figures for the first 8 weeks cited above and those for the last 36 weeks of the year.

The sterno-pubic measure shows a similar tendency to be somewhat more provocative of irritability in the early months than later, but to a much smaller degree than the sacro-pubic measure. The figures for the entire year, however, closely approximate those for the sacro-pubic. The outstanding difference is that percentages for the sterno-pubic measure are much less variable throughout the year than those for the sacro-pubic measure.

The measurement of length on the board called forth more change toward irritability than any other single measure. Almost 50 per cent of the babies under twelve weeks changed to irritability at this measure; from sixteen weeks on about 25 to 30 per cent changed. The tendency to change from irritability to contentment at this measurement was very slight.

Physical examination. Only two groups of items on the physical examination were considered worth treating as the several anthropometric groups had been. These were the percussion group, which included heart rate, percussion of the chest, palpation for the liver and spleen, and estimation of muscle tone, turgor, and subcutaneous fat; and palpation for lymph glands and thyroid. Each of these groups contained 16 items for checking. Four specific items were used also: axillary and inguinal lymph nodes, mouth and throat examination, and blood pressure.

The data for both the percussion and the lymph node groups showed an average irritability of between 10 and 25 per cent during the first 18 weeks. From 20 weeks on the irritability on the physical examination became less and less. The curves for these two groups follow fairly well the curve for total irritability on the physical examination.

During the first two weeks of daily physical examinations the babies showed a considerable tendency to fuss at the palpation for axillary and inguinal lymph nodes. From 15 to 30 per cent of the babies fussed at these measures each day, and several children fussed at them consistently from day to day, even though they did not fuss at other parts of the examination.

The percentage of children who fussed at the mouth examination was somewhat smaller. In general not more than 15 per cent of the babies were disturbed by having their mouths pried open and tongue blades thrust down their throats. In fact the babies seemed not to mind this unpleasant little incident nearly so much as do older children or adults.

Blood pressure, too, was taken with surprising ease. It elicited a fuss from almost every baby the first time, but thereafter not more than one or two babies fussed each time.

Psychological examinations. The first psychological test was grouped into two parts to be scored for irritability: in one part the baby was lying down and in the other he was sitting. The average irritability at this test was slight, and there was very little difference between irritability lying down and sitting. In the early weeks of this test about 40 per cent of the babies showed some irritability; later the number dropped to around 30 per cent. No specific item seemed to cause irritability.

The later psychological tests from six months to one year were termed the choice tests. These were scored for irritability item by item, but it was found that there was no object in the tests that consistently caused irritability in the babies. Not more than one or two babies were irritable over any single item in the tests. In many cases in which irritability was recorded a note was added that the mother left the room, that the baby bumped his head with a toy, or that he was slightly ill with a cold. Consequently it seems that the irritability was caused by something outside the test, not by the specific test object.

The motor tests, which were made at the end of each psychological examination, usually caused more irritability than the other items. From three to five babies fussed each week when they were placed on their stomachs for creeping. No baby consistently objected to this creeping test every time it was done, but one baby objected to it consistently between the ages of three and six months.

The walking tests elicited about the same amount of fussing as did the tests on the stomach. One baby always fussed at the walking tests from fifteen to forty-nine weeks. One other was so disturbed by them that they were discontinued.

APPARENT CAUSES OF IRRITABILITY AT EXAMINATIONS

In general, irritability seems to be much more a function of the baby than of the situation. No test is a consistent irritant to all babies. If we search for factors that are common to the most irritating stimuli we discover that the situations fall roughly into four classifications. First were situations in which the babies were subjected to considerable manipulation or handling. The anthropometric examinations, which caused more irritation than the psychological, entailed much more manipulation of the baby. Measurements on the board fell within this classification, and the bathing and dressing process, to which some of the babies objected, also belonged in this group. Second were situations in which certain sensitive zones were stimulated in a way that was perhaps slightly painful to the baby. Several of these sensitive zones were found on the very young babies; the axilla, the pubic and inguinal regions, and the sole of the foot seemed more sensitive than other parts of the body. As the babies grew older, however, the sensitiveness of these zones apparently decreased, although stimulation of them still called out a frown or a defense push from many of the babies. Third were occasions when the baby was somewhat ill or sleepy. Parents have always known that these conditions led to irritability in babies. Fourth were strange or new situations in which the baby was lonely, timid, or somewhat afraid. The departure of the mother occasionally started a cry, although this was rather rare and did not occur until rather late in the first year. Irritability from this source would have been greater, no doubt, if the examiners had been less familiar to the babies.

IRRITABILITY OF INDIVIDUAL CHILDREN

The correlations cited above suggest that some babies were placid and others irritable. The extent to which irritability characterized the behavior of each child is shown in Figure 2. Since this chart is drawn up for the whole year it does not show the developmental trends of the babies in the trait. The placidity and good nature of James Dalton and Larry showed up in the hospital period and remained characteristic throughout the entire year. Virginia Ruth and Maurice, on the other hand, were very irritable from the hospital period on. David and Quentin were very irritable at the early ages but reformed and became quite pleasant and happy at the examinations after 20 weeks.

The chart also shows that anthropometric measurements were the most irritating of the examinations, physical examinations next, and psychological observations the least annoying. It seems fair, then, to conclude that irritability is to a considerable degree a function of the individual child rather than of the situation in which he is placed. It seems likely, moreover, that the tendency toward either marked irritability or marked placidity is inborn. At least the tendency appeared at the earliest examination and lasted throughout the first year. It is to be regretted that the examiners were unable to get a record of the tone and quality of the cry from time to time. If their ears are to be trusted, these were quite constant for each baby throughout the year. To be sure, the baby's cry changed with age, but it changed perhaps more in pitch and loudness than in timbre. Some cries were best described as ear-piercing shrieks, others

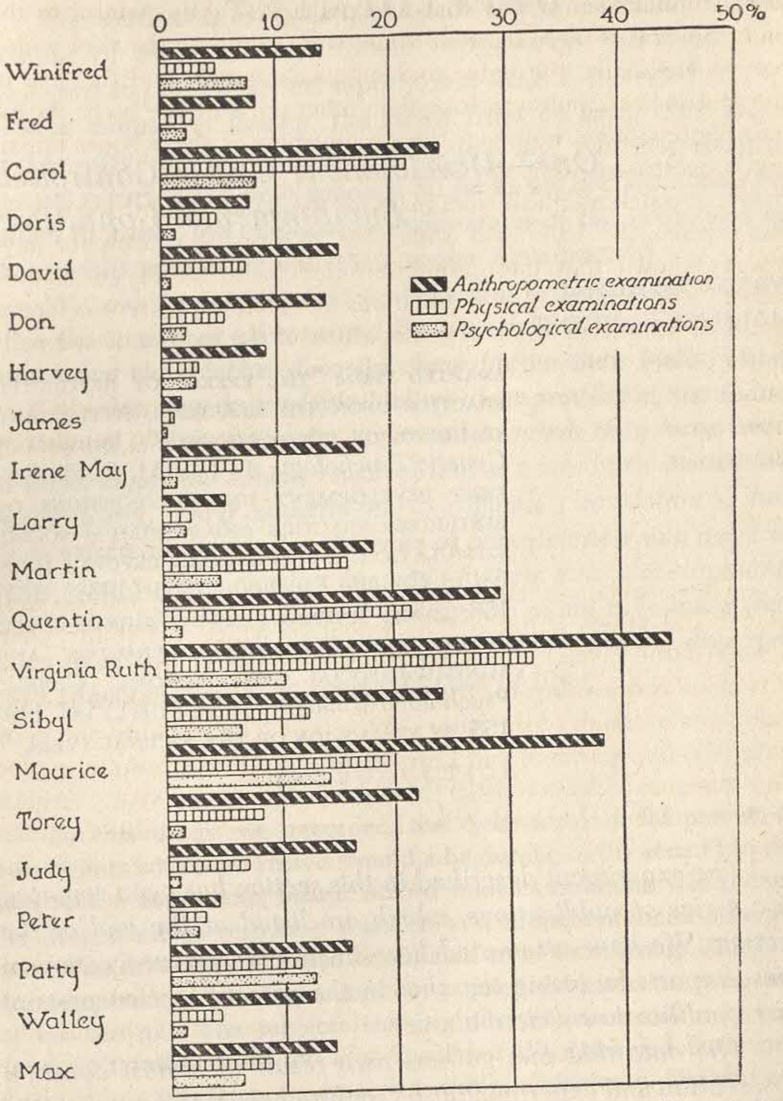


Figure 2. Irritability of individual children at the different types of examination throughout the first year. Percentage of total possible irritability score.

as plaintive wails, and still others as cries rather than screams. To discuss these qualities of the cry further might betray one into anthropomorphizing about the infants. But it is true that three babies whose early cries sounded plaintive and as if they were afraid were the three who later reformed in the matter of irritability, whereas the two whose ear-splitting yells seemed expressive of rage remained the most irritable babies in the group.

III–CAUSAL FACTORS IN EARLY DEVELOPMENT

One—Development Under Controlled Environmental Conditions

WAYNE DENNIS & MARSENA G. DENNIS

ADAPTED FROM "THE EFFECT OF RESTRICTED PRACTICE UPON THE REACHING, SITTING AND STANDING OF TWO INFANTS," *Journal of Genetic Psychology*, 47 (1935), 21-29; "INFANT DEVELOPMENT UNDER CONDITIONS OF RESTRICTED PRACTICE AND MINIMUM SOCIAL STIMULATION: A PRELIMINARY REPORT," *Journal of Genetic Psychology*, 53 (1938), 151-156; AND "INFANT DEVELOPMENT UNDER CONDITIONS OF RESTRICTED PRACTICE AND MINIMUM SOCIAL STIMULATIONS," *Genetic Psychology Monographs*, 23 (1941) 147, 149-155, BY PERMISSION OF THE JOURNAL PRESS.

The experiment described in this section has been reported in a series of publications, which are listed at the end of the section. We have attempted here to provide an integration of these reports by fitting together in the present section parts of three publications (cited above.)

The data that one gathers as a result of almost constant observation and experimentation with two infants over a period of 13 months are extremely numerous and varied. As a result, not all of our findings that might be of interest have yet been published.

Our published reports, represented here, are fullest with regard to motor development. This is true largely because it is in the motor field that previous investigators have provided standards with which we could compare the records of our subjects.

The student is likely to inquire about the emotional development of these children. Some material on this topic has appeared in our published reports, but little is included in the present summary. Briefly, the twins from an early date were very positive toward us, smiling, cooing, and laughing despite our not returning these responses. So far as we had standards by which to judge, the emotional development of the twins was as thoroughly normal as was their motor development.

We are frequently asked about the history of the twins after the time that we returned them to their mother. It will be noted that their father deserted them before their birth. Their mother, also, has shown little feeling of responsibility for them. The result is that, since the experiment ended, they have been at times under the care of relatives and at times in an institution for children. Their adjustment to difficult circumstances has been relatively good. For the sake of completeness, we hope at some time to give a fuller account of their later development, but it should be obvious that among the many influences that have affected them, it will be impossible to extricate any specific effects arising from their first year of life.

INTRODUCTION

In this study we recorded the behavioral development of two infants who were reared from the beginning of the second to the end of the fourteenth month of life under conditions which may be designated as restricted practice and minimum social stimulation. The essentials of these conditions were as follows: The two experimenters (Mrs. Dennis and the writer) exercised the sole care of the subjects. The subjects saw no other children and saw very few adults aside from the experimenters. We refrained from performing any infant responses in the subjects' presence. We did not reward or punish the subjects for any response nor instruct them in any way. Furthermore, the environment of the children was such that in respect to many responses there was little or no opportunity to engage in practice. The aim of the study was to determine what behavioral development would occur when the care of the infants was reduced to the minimum attention which would insure their comfort and well-being.

Our subjects were fraternal twins, but the relationship of the infants was not an important part of the study. Our original plan envisaged the use of only one subject, but the first inquiry for a subject to the Social Service Department of the University of Virginia Hospital revealed an opportunity to secure a pair of nonidentical female twins. The proffered twins were accepted because they enabled us to double the number of our subjects with considerably less than a doubling of cost and care. Thus the twin relationship of the subjects was, from the standpoint of our problem, irrelevant.

Their parents are of north-European ancestry. The father was a former taxi-driver, whose whereabouts at the time of the birth of the twins was unknown. The mother had been a saleswoman in department stores. The father had no other children; the mother had two children by a former husband. We were able to obtain the twins as subjects because the mother was unable to provide for them. They came to us when they were 36 days old. The mother understood that we offered temporary care of the twins in return for the privilege of studying them. She understood the general nature of our research and was cooperative at all times.

ENVIRONMENTAL CONDITIONS

Throughout the experiment the twins lived in our home but they were confined to the nursery. This was a second-floor room, so situated that from the infants' position only sky and tree tops were visible through the windows. The room itself contained only the subjects' cribs, a bureau, a table, two chairs, and a screen near the door. No picture or decoration of any sort was permitted in the nursery. The door of the room was kept closed, and we entered the room only to care for the subjects, to observe them, and to experiment with them. The twins were never taken from the nursery except on a few test occasions and for trips to the University Hospital for anthropometric and pediatric examination. On these trips the infants' faces were covered. For three months during the summer the subjects lived in a summer cabin, 17 miles away, in a room similar to the winter nursery. As the trip to and from the cabin was carefully supervised, and as their mode of life at the cabin was identical with that in town, this fact does not alter the principle that the environment of the subjects was limited and controlled in a very strict manner.

Although the infants were indoors at all times, we saw to it that fresh air and sunshine were plentifully supplied. Windows were opened for a period daily, the length of the period depending upon the outside temperature. The subjects, dressed only in shirt and diaper, were given sun baths before an open window. Direct sunlight came through the windows during a large part of the daylight hours. In spite of spending the first year of life indoors, the twins had a sun tan at all times.

The subjects were placed in individual cribs, of the trade name "Kiddie Koop." The cribs were placed side by side with a screen equal in height to the cribs between the two, so that the twins could see each other only when taken from their beds. During the first nine months the subjects were taken from the cribs only for feeding and bathing or when removal from the cribs was demanded for the purposes of experimentation.

BEHAVIOR OF THE EXPERIMENTERS

With the exception of a few occasions during the latter part of the experiment the sole care of the twins was supplied by the experimenters. This means that we bathed and fed the infants, changed the diapers and bed clothing, and cleaned the room. The infants seldom saw other people, and when they did it was with our knowledge and supervision. Visitors were required to adhere to the same practices which we imposed upon ourselves.

We carefully refrained from rewarding or punishing the subjects for any action. The subjects were not scolded or spanked or treated roughly in connection with any response. We did conduct some experiments to determine the first reaction of the subjects to scolding and to a light slap on the thigh, but these stimuli came at a time when the infants were silently and quietly watching the experimenter. With a few exceptions, we never encouraged or discouraged any act of the twins. The exceptions to this rule, and to other such general rules, occurred in the last month of the investigation, when the experimental conditions were partially suspended.

Indifferent behavior toward infants is most difficult to achieve, not in respect to avoiding such overt acts as praising and fondling but in respect to the amount of attention which is given to the infant. If the infant is doing something new or interesting, the adult is likely to watch the child much more closely than usual, and this increased attentiveness on the part of the adult may act as a reward and may induce repetition of the response. In order to avoid variation in attentiveness we attempted to watch the infants at all times when we were in their presence, except when we were taking notes. In this connection it must be borne in mind that when we were in the nursery we were never engaged in anything other than meeting the needs of the subjects and observing their behavior. Routine behavior, as well as any new behavior of the subjects, was observed and recorded each time we entered the nursery. This is very different from the ordinary home situation, where the infant receives attention primarily when he engages in some unusual, or some culturally significant, response. It is our belief that we did not reveal more interest in any one act of our subjects than we did in any other act.

This leads to a consideration of our treatment of the twins' cries. In this respect our procedure was as follows: If either of the infants cried at some time other than feeding time, one of the experimenters entered the room and investigated the cause. The cause was ordinarily a soiled diaper. The diaper was changed, and we left the room. If the child cried as we left, we paid no attention to such a cry.

The feeding routine was maintained in a general way, but not with extreme rigidity. If an infant was asleep at the regular time for feeding, we allowed her to go somewhat beyond the usual period. If she fretted a great deal or cried strongly before the hour for feeding, her food was given as much as 15 minutes early. In that sense, we may be said to have rewarded the crying of the subjects, but in so doing we could hardly have rewarded any other response. We shall see later that crying was very infrequent.

We not only avoided reward and punishment but we avoided acts which might have provided examples for imitation. With certain exceptions to be noted later our behavior in the nursery was limited to changing diapers, bathing, feeding, etc. We carefully refrained from baby talk and from babbling, as we wanted to know whether such vocalizations would occur without example. Likewise, we never performed for the twins such acts as patting their hands or playing with their toes.

EARLY RESTRICTIONS

Thus far we have spoken only of the conditions which remained relatively constant until the last few weeks of the experiment. We turn now to more stringent restrictions in the environment of the subjects which, in the main, were applied only for the first half-year. The conditions to be described were designed to provide answers to specific questions and were abandoned when the answers were obtained.

We wished to determine whether or not the infants would smile upon hearing the voice of the adult, if speech were not associated with the care and attention which the adult supplied. For this reason, until the twins were 26 weeks of age, we never announced our entry into the nursery and never spoke to the subjects. We were not totally silent, for we occasionally commented to each other while in the nursery, but we were careful not to make comments while we were feeding or otherwise caring for the twins. Our speech when we were outside the nursery could be heard by the infants, but it had no more relation to their behavior than did traffic noises or other common sounds.

We wished also to know whether positive responses toward us would develop if we refrained from smiling at the twins and from petting, cuddling, and fondling them. In order to determine the answer to this question we avoided these expressions during the first 26 weeks. Withholding of demonstrations of affection of this sort was not an easy task to impose upon ourselves, particularly as the subjects themselves were very expressive. From the 15th week onward they almost invariably greeted us with a smile and a vocalization. After this fact was thoroughly established, we decided in Week 27 to return their smile of greeting, and to speak to them as we approached.

The reader may ask how we could keep from rewarding various responses of the subjects when once we permitted ourselves to smile at and to fondle and play with the twins. This was accomplished by engaging in these responses only at times when no particular act of the twins could be said to be encouraged by our behavior. We romped with the children, as by shaking or rolling them only when they were indifferently employed with some response that was already well-established.

During the 36th week we began placing the twins for several minutes each day upon quilted pads on the floor. This was for the purpose of giving them some experience on a relatively hard surface. At the beginning of the 42nd week we started placing the infants in high-chairs for a short period each day. But even in the last month of the experiment, the larger part of the twins' day was spent in the cribs.

No toys were provided for the subjects until the 49th week. At that time a rattle and a few other toys were introduced. Our aim in introducing the toys was to see what use would be made of them when no example, encouragement, or instruction was given by the adult.

One sort of restriction to which the infants were subjected requires special comment. In accordance with our general policy of letting the twins alone and of refraining from initiating any development, we did not put the twins into sitting or standing postures, nor provide any practice in reaching for many months. Under normal circumstances parents prop the child up before he can sit alone, hold him upright on the adult's lap before he can stand well, and dangle objects before him before he can reach with any accuracy. These responses, more than any others, require the intervention of the adult in order that early practice be made possible. Our experiment ruled out nearly all possibilities of early practice of these reactions, whereas many other responses were practiced by the twins because unlike the responses mentioned above they did not require the aid of the adult.

The three responses—sitting alone, standing with support, and reaching for a dangling object—were submitted to tests when the twins were of an age such that the responses are present in all normal infants of normal environment. When these tests had been made, we no longer refrained from placing the twins in sitting and standing postures, nor did we keep all movable objects out of their reach.

FURTHER DESCRIPTION OF CARE

We attempted to give the two subjects identical treatment. This was carried to the extent of dividing the attention of each experimenter equally between the two subjects. For convenience, the experimenters will be referred to by their initials: *W* referring to the writer and *M* referring to Mrs. Dennis. *M* fed Rey throughout one day while *W* attended to the feeding of Del. During the next day, *M* fed Del and Rey was fed by *W*, and so on. The infants were bathed one at a time, but the order of bathing was reversed from day to day. The bathing was done by *W*, the drying and dressing by *M*.

In order that the infants' positions in the room should not lead to a difference in treatment, the crib which Rey occupied during one day was occupied by Del during the next day, and so on. Thus it will be seen that great pains were taken to insure that the two subjects were treated exactly alike.

A few words may be added on miscellaneous aspects of the twins' care. Before the twins came to us, the mother's milk had been found to be quite inadequate, and supplementary feeding by bottle had been resorted to. At the beginning of the experiment, bottle feeding constituted the sole means of feeding. The nursing bottle was utilized for the giving of liquid foods throughout the experiment, that is, weaning from the bottle was not attempted, but solid and semisolid foods were given by spoon.

The diet of the subjects was chosen with the advice of Dr. L. T.

Royster of the Pediatrics Department of the University of Virginia Hospital. During the early months the diet of the twins consisted of lactic acid milk, which was kindly prepared for us by the Hospital staff, and of orange juice. Soup, cereal, and strained fruits and vegetables were introduced gradually after the 24th week. No elimination training of any kind was undertaken.

The major part of our records consist of day-by-day observations of behavior which fill about 1,800 pages of notebooks. Notes were taken nearly every time either experimenter entered the room to care for the babies. In addition to entrances into the room which were made in order to care for the infants and to experiment with them, several entrances per day were made solely to note down the condition and activities of the babies. Habitual as well as new responses were recorded. On the average, notes were written 11.88 times per day. A feeding period or an experiment, which may have lasted for half an hour or more and during which notes may have been taken continuously or on several occasions, was counted as only one note-taking in computing this average. Allowing 12 hours of undisturbed night sleep for the babies, we obtained, on the average, one record of activity during each of the remaining hours. We estimate that one or both experimenters were in the room during about two hours per day. As a corollary it should be pointed out that this means that the babies were not "isolated."

The notebooks contain observations not only of the spontaneous activities of the twins but also records of their reactions in test situations. These notes include behavior upon first being placed in a sitting position, and upon first being given an opportunity to stand —tests which were mentioned above—and also behavior in a number of other tests.

GENERAL BEHAVORIAL DEVELOPMENT

We have recently compiled the developmental records of 40 subjects of baby biographies (10). The reader is referred to the original publication for definitions of the items of behavior, as they are too long to warrant repetition in full.

In Figure 1 the notations along the left margin are short descriptions of the behavior items. The horizontal line opposite each notation indicates the age range during which this item made its appearance in the biographies and the short vertical which cuts each horizontal line indicates the median age for the appearance of that item. The figures at the left of the chart indicate the number of reports of each item. All of the infants whose records are on this

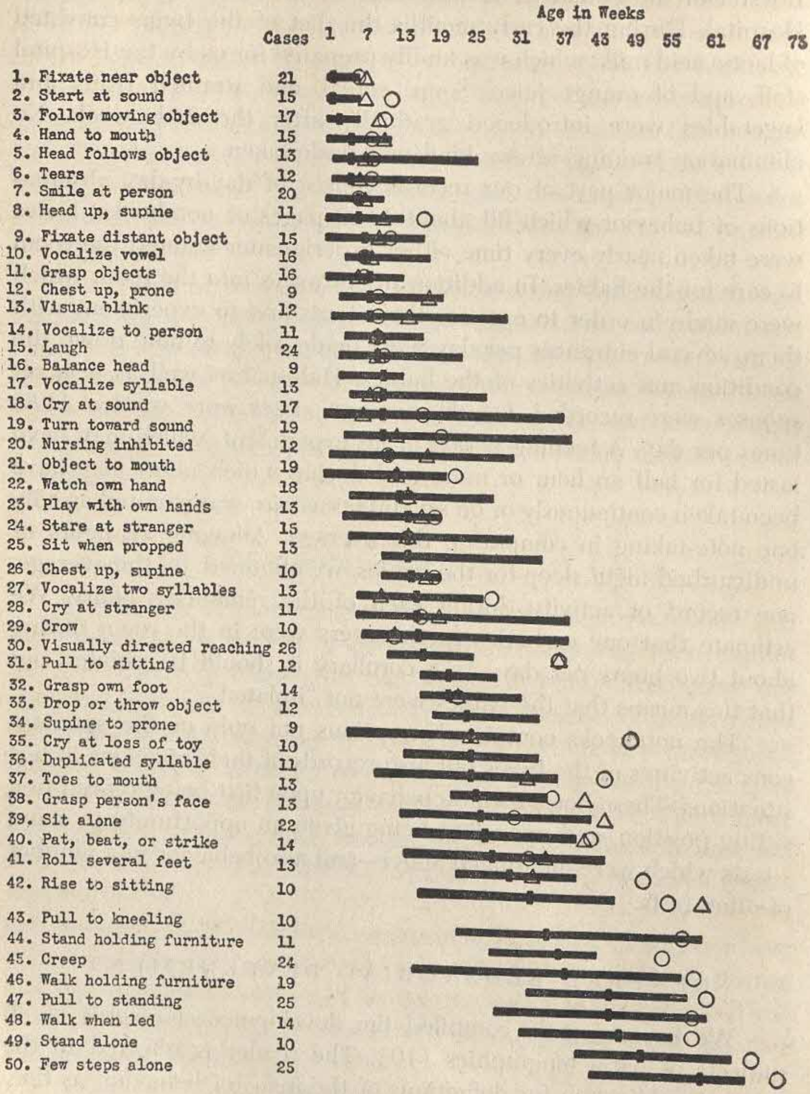

Age in Weeks
Cases
1 7 13 19 25 31 37 43 49 55 61 67 73
1. Fixate near object 21
2. Start at sound 15
3. Follow moving object 17
4. Hand to mouth 15
5. Head follows object 13
6. Tears 12
7. Smile at person 20
8. Head up, supine 11
9. Fixate distant object 15
10. Vocalize vowel 16
11. Grasp objects 16
12. Chest up, prone 9
13. Visual blink 12
14. Vocalize to person 11
15. Laugh 24
16. Balance head 9
17. Vocalize syllable 13
18. Cry at sound 17
19. Turn toward sound 19
20. Nursing inhibited 12
21. Object to mouth 19
22. Watch own hand 18
23. Play with own hands 13
24. Stare at stranger 15
25. Sit when propped 13
26. Chest up, supine 10
27. Vocalize two syllables 13
28. Cry at stranger 11
29. Crow 10
30. Visually directed reaching 26
31. Pull to sitting 12
32. Grasp own foot 14
33. Drop or throw object 12
34. Supine to prone 9
35. Cry at loss of toy 10
36. Duplicated syllable 11
37. Toes to mouth 13
38. Grasp person's face 13
39. Sit alone 22
40. Pat, beat, or strike 14
41. Roll several feet 13
42. Rise to sitting 10
43. Pull to kneeling 10
44. Stand holding furniture 11
45. Creep 24
46. Walk holding furniture 19
47. Pull to standing 25
48. Walk when led 14
49. Stand alone 10
50. Few steps alone 25

Figure 1.

chart were described as healthy and of normal intelligence, and none was very unusual in the amount of training which was given it. Since less than 40 records, and in some cases only 9 records, are represented for each item, it is altogether likely that the ranges here represented are too narrow.

The circles and triangles show the ages at which each of our subjects first performed the items listed on the chart. The circles indicate the records of the larger twin, named Rey, and the triangles the records of Del, the smaller of the two. A few items show no records for our subjects because in these respects we failed to make observations comparable to the biographical reports.

By referring to the chart, it will be seen that with respect to the items whose upper limits appear within the first seven lunar months, the twin subjects with few exceptions fall within the age range of children with normal environments. In the upper half of the chart, the items in which the subjects fall beyond the range of the comparison subjects are chiefly the first three items, which occur at such an early age that it seems unlikely that the retardation of the twins was due to the experiment. It seems more likely that this may have been due to the poor nutritional condition of the infants when we received them. Aside from the first three items, the only other apparent retardations among the items which ordinarily appear within the first seven months occur in Rey's case only. They are "head up, supine," "object to mouth," and "vocalize two syllables." In none of these cases is Rey more than two weeks beyond the upper range of the biographical subjects, and since records are available in some instances for less than 20 infants, there is a strong likelihood that the ranges here shown are too narrow. It is concluded therefore that the records of our subjects within the first seven months are not distinguishable from those of the children whose records have been kept by biographers.

A similar conclusion is reached if instead of the biographical data, Shirley's recent data (7) are used for comparison. Shirley followed a number of children through the first two years of life, obtaining a longitudinal record of behavioral development. We have selected from her tables those behavior items which do not involve special tests, and hence which are comparable to our observational account of development. These are shown in Figure 2, which is constructed in the same way as Figure 1. In respect to the 14 items in this chart whose upper ranges fall within the first seven lunar months, our subjects' only retardation is a one-week delay in "startled by sound" on the part of Rey.

Such comparisons are of course not ideal, as the different sets of data were gathered by different people and in somewhat different ways. Nevertheless, it is believed that they are sufficient to show clearly that no striking abnormalities of development occurred during the period when the environment of the subjects of the experiment was highly restricted.

It is of course quite possible that had conditions been normal our subjects might have developed any one of the items at an age different from that at which it actually appeared.

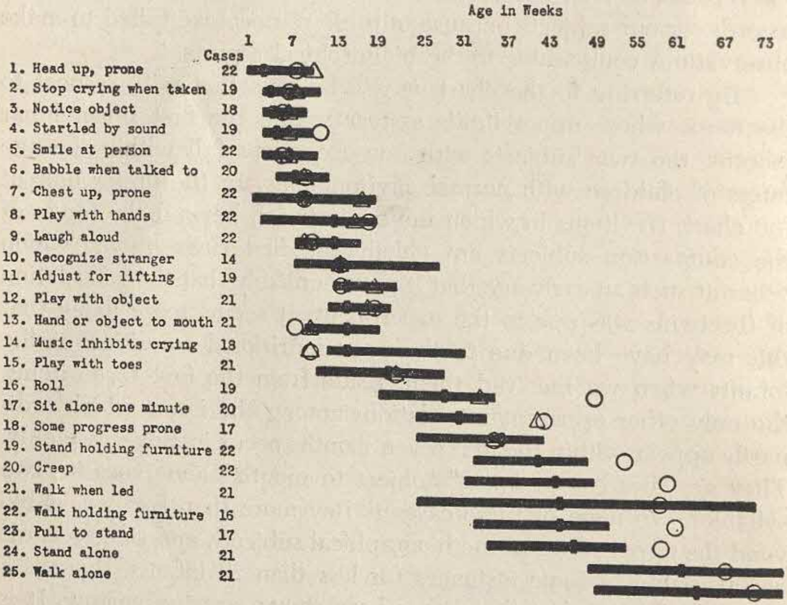


Figure 2.

That slight differences in behavior may have been induced by the experiment, there is no intent to deny, and no means to prove or disprove. But it must be concluded that in the first seven months whatever the effects of the experiment may have been, they were not sufficient to force our subjects beyond the range of infants reared under normal conditions.

SPECIAL TESTS

Reaching and Grasping a Visual Stimulus.

Reaching and grasping cannot be removed entirely from the influence of practice because the infant always has opportunities to reach his own extremities and to reach his bed and bed clothes.

Playing with the bed clothes did occur to some extent, and the infants of course played with their own hands. They also grasped their feet and occasionally the experimenter's hand as he administered a stimulus. This grasping may have been either visual or nonvisual and it is unfortunate that we did not investigate its nature. However that may be, our tests show a retardation in reaching for a dangling ring. A dangling ring was presented once per day for 40 days beginning with day 245. On each trial it was held above but within easy reach of the infant. The duration of each trial was 30 seconds. The child lay on her back in the crib. Gesell's norms (11) show that at six months (180 days) 65 per cent to 84 per cent of infants grasp a dangling ring. According to Shirley (15), the calipers used in her tests were reached for by 100 per cent of her subjects at 154 days and grasped by 92 per cent at that age. While further norms are desirable, we feel fairly certain that with normal experience our subjects would have reached for a dangling ring long before day 245. Yet Rey never reached for the ring until the 13th presentation and Del not until the 16th presentation. While the ring was fixated at every trial no attempts whatsoever at reaching occurred until the trials named above. In the case of each baby the first reaching had the appearance of developing out of some other activity. On Rey's first reach her hands came toward the ring with her fists *closed,* her left fist touched the ring, it then opened, pursued and grasped the ring and brought it to her mouth. On Del's first attempt, her hands came toward the ring *clasped together*. Her clasped hands touched the ring. Thereupon they unclasped, her right hand pursued the ring which had been set in motion by the contact, grasped it, and brought it to her mouth. Del reached and grasped the ring fairly accurately and quickly with her right hand in 20 successive trials following her first success. In the last three tests she paid little attention to the ring and did not reach. Rey, after her success on the 13th trial made no attempt to reach again until the 19th, when she grasped the ring directly with her right hand and did not move her left, which had brought the first success. On the twentieth trial she missed once with her right and then succeeded in the second reach with her right. She attained the ring readily in each of 20 consecutive trials thereafter, using her hands without preference. We must conclude that the early failures were due to the deprivation of practice. However, the speedy establishment of reaching when once it was attempted is worthy of note. The fact that the ring was pursued when it moved indicates that the reaching was visually controlled.

Sitting Alone

Sitting alone means the unaided maintenance of a sitting position when placed in it by the experimenter and *when passive balance is slightly disturbed.* It is important that this italicized phrase be included. A child who stiffens his spine when placed sitting may remain upright several seconds merely through passive balance. Very likely it is this kind of sitting alone, rather than real balancing, which is occasionally present in the newborn (2). There is as yet no reason to believe that real sitting is ever present at birth.

The most extensive norms for sitting alone are those of M. C. Jones (12). She recorded whether or not her subjects could sit alone for one minute. We are interested only in the upper age limits of this ability. Jones tested 28 children between days 270-289; of these only two failed to pass the test. Of 21 cases beyond day 290, every one succeeded.

In Shirley's group (15) of 25 children, the upper range for sitting alone was the 35th week (245-252 days) and among seven biographical studies analyzed by her it was the 39th week (272-280 days). Gesell (11) states that sitting alone is almost universal among normal children at nine months (270 days). Linfert and Hierholzer (13) found that all of their subjects sat alone at nine months.

Regardless of the lack of a common precise definition, the studies agree very well in showing that practically every child can sit alone at about two hundred and seventy days of age.

On day 263 Rey was placed in the sitting position on a pad on the floor (her legs about 60 degrees apart). When she inclined slightly backward or to either side, she made not the least observable effort to balance herself but fell toward the floor. (Naturally we caught her before she hit the floor unless the tumble was a very gentle one.) This result was confirmed many times on that day. However, when she fell *forward* her face was permitted to rest on the floor and she pushed herself upward with the arms and remained supporting herself with her arms in a frog-like posture. At the end of three minutes we picked her up. Subsequent tests confirmed the finding that she could support herself in this manner but in no other.

We were at first astonished as we had not expected that sitting, even though present, would be accomplished in this fashion, and our first impression was that the response was a totally new one. (None of the normative studies mention this response but since the completion of the experiment we have seen photographs of other children in this posture.) A little reflection, however, showed that pushing the head and chest upward from the floor with the arms was a well-practiced reaction. She had been tested many times for the "chest-up" response (15), and she had been placed prone on several occasions for exercise because we saw no relationship between such practice and the responses which we wished subsequently to test. The performance on day 263 seems therefore to be the transfer of a learned response to a slightly new situation, the only difference between chest-up and "frog-sitting" being that the legs were behind the trunk in the former and under the trunk in the latter. That this action can hardly be called true sitting is shown by the fact that in it the body was inclined far forward, and that when Rey was placed sitting fully erect she exhibited no tendency to balance herself. Although she was tested almost daily from day 263 onward, Rey first sat for a few seconds with no support from her arms on day 326, a date far beyond the upper age range for this act in other children. By the end of the first year she could sit alone for many minutes.

Del showed a similar course of development. Tested on day 262 she fell to either side, backward, or forward as did Rey. When first permitted to lie with her face on the floor she did not push with her arms but cried instead. On a second trial a little later in the day she pushed herself upward slightly but started crying after 3 seconds and was picked up. Her improvement in sitting was gradual, but she sat for 20 seconds without arm support on day 298. We conclude that neither subject came within the age range of nonexperimental or control subjects, and hence that the retardation of the subjects was attributable to the experiment. The retardation was only transitory, and the twins soon sat as well as any child.

Standing with Support

Standing with support or supporting the body weight on the feet means that the infant supports his entire weight when balanced by an external agency (usually by being grasped under the arms by the experimenter) and that this stiffening of his legs is really dependent upon his upright position and upon the contact of his feet with a surface. The latter clause is included because some might think it foolish to ask whether post-natal practice is essential to support of body weight since standing with support is sometimes reported in the newborn (2). We are inclined to believe that the so-called standing of the newborn, as well as the "stepping movements" of the newborn, have little relation to the infant's position or to the surface with which he is in contact. They may be mere "strong leg extension" and "alternating leg movements."

In our tests we grasped the infant in the region of the armpits and maintained the child erect, brought her feet slowly to the floor, and let her sink to the floor if she did not support her entire weight. A partial support of the body weight by the child could be felt as a lessening of the burden in our hands. Gesell (11) finds that this test brings forth a pushing reaction of the feet in 85 to 100 per cent of the cases at four months, although full support of the body is rarely achieved at that time. Linfert and Hierholzer (13) find full support present in 10 per cent, 92 per cent and 100 per cent at six, nine and twelve months respectively. (Fifty infants were tested at each age level by Gesell and by Linfert and Hierholzer.) The latest age for this performance in the 25 infants studied by Shirley was thirty-seven weeks.

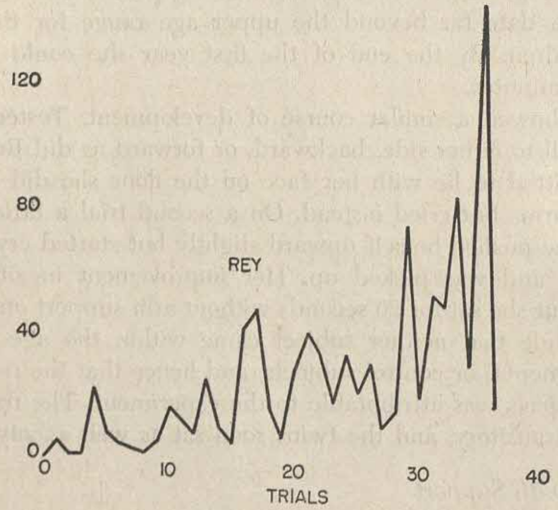


Figure 3.

Our subjects were tested for standing 20 times in the fourth month. While there was a slight push with the legs in about one-fourth of the tests, it was momentary and never resulted in supporting the entire weight of the child. Extensor thrusts may have occurred just as often when the child lay on her back although unfortunately we did not investigate that possibility. Such tests were again given each subject on day 364. The results of the first of the delayed tests were essentially the same as those obtained eight months earlier. In the first 10 tests, neither baby supported her own weight. Del gave a slight push in all 10 tests, Rey in only 4. The pushes were of a momentary character which caused the infant to bounce up and down but not to stand. Neither infant steadily supported her weight for even a fraction of a second.

A few minutes after the first tests on day 364 ten tests of a slightly different sort were given each subject. The infant was held as before except that we inclined each subject's head and chest far forward. This was done in order to determine whether there was any tendency for the child to bring her body into an upright position at the moment the leg extension occurred. The tests showed no such tendency.

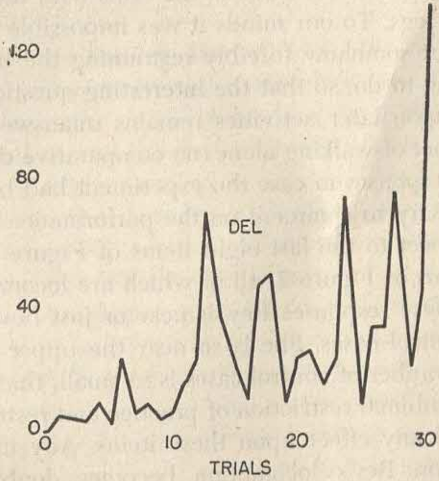


Figure 4.

In order to find how rapidly the infants would learn to stand with support, many trials (with the trunk upright) were given during the remainder of day 364 and on days 365, 366, and 367. No motivation for standing was given unless it be said that the pressure of the body upon the folded legs when the weight was not supported served as a motive. The first momentary support of the body occurred on day 364 for each subject, only about 8 hours after the first failure. When the length of the support grew to a measurable interval the duration of support on each trial was noted by a stop watch. Figures 3 and 4 show the progress beyond that point. The improvement in standing shown in the graphs was gradual and irregular, but before the close of day 367 each infant had stood continuously for two minutes or longer. We must interpret standing as we did sitting, that is, the experimental conditions retarded the occurrence of the initial performance, although this retardation was soon overcome.

Subsequent Locomotor Development

Shortly after the tests just described, the infants became able to achieve the sitting position in their cribs of their own accord. (Unfortunately there are no norms on this performance, but it seems likely that the twins were retarded in this respect also. Rey, day 387; Del, day 423.) Once they were able to raise themselves to a sitting position, they could grasp the top of the cribs, make attempts to pull themselves to standing, and could also obtain practice toward standing by pushing against the sides of the cribs with their hands while stiffening their legs. To our minds it was impossible to prevent this practice without somehow forcibly restraining the babies. This we were not willing to do, so that the interesting question of the effect of restriction upon later activities remains unanswered. Moreover, except for the act of walking alone, no comparative data were available for later responses in case the experiment had been prolonged.

It is necessary to comment on the performance of each of our subjects in respect to the last eight items of Figure 1 and the corresponding items of Figure 2, all of which are locomotor in character. In all of these responses Rey is near or just beyond the upper limit of the control cases. She is so near the upper extreme, however, and the number of control cases is so small, that it is uncertain whether the combined restriction of practice and restriction of social stimulation had any effect upon these items. Any influence of the experiment upon Rey's locomotion becomes doubtful when we widen our sampling of normal children. The upper ranges of Shirley's study are slightly beyond the biographical ranges, and Rey's record is in many respects indistinguishable from the slowest of Shirley's cases, who of course were not deprived of social encouragement or practice. (This fact is not shown clearly by Figure 2 because Shirley was not able to obtain complete data for her most retarded subject.)

On the other hand, Del's locomotor performances in the items at the bottom of the chart occurred at ages considerably beyond the upper limits set by the biographical records or by other studies of normal children. They are too retarded to be presented on a chart of this width and in consequence are shown in Table I. The last of these items, which is walking alone, occurred only shortly before the second birthday.

Certain considerations make it extremely doubtful that this retardation was due to the experiment. The subjects of the experiment are now slightly more than six and one-half years old. While Rey throughout her history has been essentially normal, it has become apparent during the past two years that Del has some dis-

TABLE I

DEL'S LOCOMOTOR ITEMS

	Weeks of Age
Pull to kneeling	77
Stand holding furniture	67
Creep	never
Walk holding furniture	92
Pull to standing	81
Walk when led	81
Stand alone	111
Few steps alone	111

ability of her left arm and leg. Her left arm is much more awkward than her right in all respects and is almost unused by her. Her left leg is very inferior to the right. Del has been submitted to a thorough clinical examination, and the medical report shows no anomalies of bone or muscle and gives a diagnosis of mild left hemiplegia probably referable to cortical injury at birth. This injury may also be the reason for Del's low IQ, which at four and one-half years was 70 while that of Rey was 107 (Stanford-Binet).

Neither the hemiplegia nor the low IQ of Del made themselves manifest at an early age. It will be seen upon examination of Figure 1 that Del was retarded in practically nothing prior to nine months of age. Extensive movie records taken in the third month reveal no disability on the left side. These facts of course agree well with the view that the cortex is very incomplete at that age, and with the recent findings that later intelligence cannot be predicted within the first year (1).

While the special facts of Del's case deserve attention in themselves, it must be kept in mind that they are relevant in the present connection only because they tend to show that the retardation which appeared following the first year was in all likelihood due not to the experiment but to some organic deficiency.

AUTOGENOUS BEHAVIOR

Our chief interest in conducting the experiment lay in determining the responses which a child could develop if he were removed as far as is possible from the influence of adults and of other children. This is an old query, having been asked several centuries B.C. by Psametichus who reputedly reared two children apart from speaking adults in order to determine whether or not they would develop language in the absence of adult example. The question as to what the unaided individual would be able to do was the basis also of interest in the so-called "wild children" who have been found from time to time, the most famous of which was Victor, the wild boy of Aveyron, who was studied and trained by Itard. Since there has been a prolonged interest in knowing what responses the individual is able to achieve when uninstructed, it is surprising that no term by means of which we can designate such responses has come into general use in psychology.

The term "instinctive response" immediately comes to mind. To be sure, this term has been accorded a variety of meanings so that it would not be difficult to find one usage which approximates our present need. But there are many objections to the use of "instinct" in this sense. There is a tendency at the present time to limit the term "instinctive response" to the connotation of a reaction pattern which requires no learning, and it seems best to further the acceptance of this usage. We wish a term which will not only include "unlearned response" as defined above but which will also include other responses which are developed by the individual independently of other persons, whether by discovery, by trial and error, or by whatever means. We wish a term which will designate instinctive responses and in addition responses which are independently acquired.

We suggest that such responses be called "autogenous." This word already has a recognized place in the vocabulary of several sciences, and is capable of useful application in describing behavioral development. The general meaning given for autogenous in Webster's *New International Dictionary* is "self-generated; produced independently of external aid; endogenous." In medicine the term is used to refer to some condition which originates within the organism as opposed to a condition which results from contagion. In anatomy, autogenous is employed to refer to ossification which proceeds from an independent center. In zoölogical usage, the term refers to a condition which originates within the organism. These meanings will be seen to be in line with the one suggested above for the description of behavioral development.

The present study has shown to what an extent autogenous behavior is characteristic of infancy. While a child cannot be excluded from all contacts with other persons, we feel that in the present experiment our contacts with the subjects did not provide the *origin* of the new responses which were recorded. We avoided the use of reward and punishment. We refrained from performing the infantile responses which the twins added to their repertoire. From our experiment it appears that practically all of the behavior of the first year of life is autogenous. The diary account of development gives an impression of behavior as extensive, as varied, and as typically human as does a biography of an infant reared under normal home conditions. It was shown that the twins exhibited all of the responses which are commonly recorded by the biographical method, and also all of those which Shirley found to be characteristic of her subjects. The extensiveness of the autogenous repertoire is indicated by the fact that we were able to list 154 new responses which occurred in the course of the experiment.

No doubt the introduction of training might have increased slightly the number of new responses. The additional reactions would have been such items as waving the hands when the adult said "*bye-bye,*" of kissing and of throwing kisses, and of pointing to objects when they were named. Such responses are obviously sociogenous, and have been considered so for centuries because they never appear until after appropriate training has been administered. But it is likely that the number of sociogenous responses which can be acquired by a child under one year of age is very small. They are in no sense an essential part of normal development during the first year, and since they are unusual at that period they do not constitute an exception to the statement that our subjects autogenously exhibited all of the common responses which are characteristic of infants in the period preceding the first birthday.

THE QUESTION OF UNLEARNED RESPONSES

How many of the autogenous responses of the first year are instinctive? Before we can answer this question we must determine how one is to discover instinctive responses and we must then canvass our data from this point of view.

As used here, the terms "instinctive response" and "unlearned response" are synonymous. We explicitly exclude from the discussion all other connotations which have been associated with the word "instinct."

An unlearned response is usually defined in negative terms as a reaction which has not been learned. Since we have no universal agreement as to what constitutes learning, and no infallible indication of its contribution to the development of a response, it follows that we do not have an operational method of identifying instinctive responses.

This much, however, is probably agreed upon by all students of behavior, that if it were rendered impossible for an organism to display responses before a given moment—as by administration of an anesthetic—that all the responses which appeared upon the organism's first opportunity to react might be called unlearned responses. In other words, it is commonly agreed that learning cannot occur in the absence of a reaction history, and hence all responses which appear coincidentally with, or shortly following, the onset of motility may be considered instinctive. But since it is not reasonable experimentally to render the human fetus or the human infant immobile, this criterion of an instinctive response has no applicability to man.

A second basis on which some agreement might be reached has reference to the *kind* of reaction history which precedes a specified response. We are interested to know whether a response suddenly appears in full or whether its final form is achieved only after a series of approximations. If a response occurs suddenly, without any forerunners which resemble it, we are inclined to think that it may be unlearned. However, we cannot be certain of this interpretation. A response may develop through learning without our being able to recognize this fact. For instance, if typical human laughter occurs in an infant who has never laughed before, it is obvious that laughter has not been achieved by the gradual perfection of responses which could be recognized as attempts to laugh. Nevertheless, laughter may have been attained by virtue of the fact that the child earlier engaged in smiling, in certain laryngeal responses, and in a variety of types of breathing. In the development of laughter, the child might thoroughly practice every element of laughter without our being able to detect by ordinary observation any rôle of practice in the execution of the first complete laugh, since the practice would not result in the production of sound if the elements were practiced separately.

Even if for the present we disclaim the ability to detect an instinctive response we may still retain a legitimate interest in the *ontogenetic history* of responses. We may ask which reactions make their appearance with little apparent contribution from the previous reactions of the individual. Such responses would seem the most likely to be truly unlearned.

SOME NEGATIVE RESULTS

Evidence has been presented to show that both of the twins were retarded in their establishment of visually directed reaching and grasping, sitting alone, and standing with support. Further, it has been shown that when training was instituted reaching and standing were established with relative promptness; sitting alone developed more slowly.

The explanation of these facts which we favor is that the retardation was due to the lack of the usual learning situations. Maturation alone, however it be defined, was insufficient.

Other possible interpretations will no doubt suggest themselves and must be considered. The suggestion which we have received most often in relating the experiment orally is that the infants were improperly exercised and hence too weak to perform the required responses although the neural patterns were present. This suggestion has never been made by anyone who saw the twins in action. As a matter of fact the infants received a great deal of exercise. While it is true that prior to certain dates the infants did not exercise in the sitting position, in the standing position, and so on, they were, like all infants, almost continuously active. A time-sampling study of crib behavior was conducted from days 185 to 240. The observational periods were 15 seconds in length and each infant was observed a total of approximately 1,200 such periods when awake soon after a feeding. There were, on the average, approximately 3.5 *kinds* of movements per 15-second period per baby. The infants were in the best of health, were not rachitic, and their muscles were large and firm. Nevertheless, it may be suggested that while they were muscularly strong in many respects, yet the muscles involved in the performances which were tested were undeveloped. Two facts speak against any view which attributes the retardation of the infants to weakness of particular muscles rather than to the absence of proper innervation of these muscles. The first is that observation of the infants in the early test trials did not reveal any attempts to perform responses which they were unable from weakness to carry through, as a child who has been ill may attempt to perform an action which he is too weak to accomplish. Secondly, grasping and standing were established in a very short time. There is no reason to believe that muscular strength may be developed with such speed. Rather it would be expected that much effort within a short period of time would fatigue the muscles.

Another possible interpretation of the retardation is that habits contradictory to the expected responses had been set up. So far as we could observe, the infants performed no response in the initial sitting tests except that of supporting themselves with their arms, and none in the first standing tests except for momentary extensor thrusts. Neither these responses nor the responses to the dangling ring seem incompatible with the response whose presence was being tested.

One must consider also the possibility that the reactions which we studied, although instinctive, waned of disuse before the proper tests were made. Each of the responses which has been examined was tested shortly before or after the expiration of the usual upper age extreme. If the instincts waned, they must have done so in short order. But even if the infants had been tested daily (which, by providing practice, would have defeated the purpose of the experiment) it might be urged that the instinct ripened and waned from lack of practice between two tests. Until some evidence of waning is advanced, there is no reason to put much faith in this very flexible hypothesis.

That the infants were retarded in the activities by organic motor disabilities is contra-indicated by the fact that they learned readily to sit and to stand and to reach when training was begun. In fine, all other possible interpretations seem ruled out and it seems necessary to conclude that the twins were retarded in sitting alone, standing with support, and reaching and grasping because of the restriction of learning opportunities.

SOME POSITIVE EVIDENCE

Three responses appeared which seemed not to be derived from the antecedent reactions of the subjects. The first of these responses was laughter. The other two we shall tentatively call "attempting to rise" and "attempting to turn," although in greater strictness they might be labelled "lift head and shoulders when supine" and "rotate head and shoulders when supine." We shall review the history of each of these actions.

Laughter

Rey's first clear-cut laughter occurred in Week 21, while Del's first unquestionable laugh was heard in the 16th week. Since each infant smiled and vocalized long before these dates, it is possible that the exercise of these two functions had an influence upon the development of laughter. However, only twice in the case of each subject was any sound resembling laughter heard prior to the date of the first unmistakable laugh. These sounds, which will be called near-laughs, were Weeks 10 and 13 in Rey's case and in Weeks 9 and 10 in Del's case. The absence of any other record of near-laughter prior to full-fledged laughter is all the more striking because our observations of sounds were not limited to the time which we spent in the babies' room. We could hear the vocalizations of the infants with ease even when we were not in the nursery.

The occurrence of two sounds somewhat similar to but not identical with laughter can hardly be said to be favorable to the view that laughter develops through practice.

"Attempting to Rise"

The second response which appeared as if without practice was the raising of the head when the child lay on her back. Often this behavior was engaged in persistently as if its full goal had not been attained. From its inception, it had the appearance of a true attempt to attain the sitting position although neither child could well have been acting from any such conscious motive at the time of the first trials. Rey's performance of this response was first observed in the 16th week and Del's in the 10th. The response occurred from time to time and gradually became stronger. By the 18th week Rey was able to raise her shoulders as well as her head above the level of the mattress; Del did this a week earlier than did Rey. Nevertheless it was only in the 61st and the 56th weeks, respectively, that the two infants were able to attain the sitting posture without assistance. It is more than likely that their many attemps to sit were a real factor in the final achievement of this response, but we are at a loss to know how learning could have contributed to the first lifting of the head from the pillow, and to know why the activity was persisted in when it seemed to achieve nothing.

"Attempting to Turn"

The third response which gave the impression of being unlearned was the first instance of turning vigorously toward one side while supine.

Del on a day in the 12th week worked continously for 20 minutes in an apparent effort to turn over from the supine to the prone position. Her efforts did not succeed in changing her position and in the end she cried. Rey in Week 16 made a similar but less strenuous and less persistent effort which also was unsuccessful. It did not result in crying. In these performances the infants' shoulders and head were repeatedly rotated to a great degree. There was no suggestion that the subjects were trying to reach anything or to get a better view of anything. Nothing external seemed to control the reactions. While rotations of the head and slight shoulder rotations had occurred before this time, the performances just described were definitely novel in their extent and their persistence. The stimulus to these reactions may have been discomfort engendered by lying continuously in one position, but whatever the stimulus, it, in itself, cannot account for the patterning of the behavior which resulted from it.

These actions, which we have called attempts at turning, were not repeated by either baby—at least not while they were under observation—until several weeks had elapsed. With Rey the second attempt was seen in Week 24, with Del, in Week 22. Del's third known attempt, in Week 24, was a success, but she cried strongly after she had lain in the newly-achieved prone position for only 30 seconds. She did not turn again from supine to prone until Week 28, and this time also the performance was followed by crying. Crying did not recur in this situation thereafter. Her skill in turning over gradually improved, so that by Week 35 she was accomplishing the feat nearly every day.

Rey was never seen attempting to turn over after the failure recorded in Week 24 until Week 35. Her first success was not until the 50th week. Subsequent to this, turning from supine to prone became a frequent response.

MEAGERNESS OF THE EVIDENCE FOR POSTNATAL INSTINCTS

In view of the large number of developmental items, it is significant that we saw only three responses whose origin did not seem to lie in the slow modification of antecedent activities. The trend of our records is toward ascribing only a very small unlearned element to those additions to the infant's repertoire which occurred in the course of the experiment. We observed only three responses which appeared suddenly as if from the maturation of some bodily mechanism. Although these responses seemed to appear "out of the blue," we must admit the possibility that movements previously made may have contributed to the appearance of these responses. It is also possible that these responses may have had precursors before birth, or between birth and the beginnings of the experiment, or when we were not observing the subjects. For these reasons, we cannot be dogmatic with regard to the mode of development of these activities.

All other responses which we studied presented a picture of a gradual acquisition, and hence at least *simulated* learning. It is possible, of course, that growth processes may affect behavior in the same manner as does learning and hence the gradual improvement of coördinations may give no clue as to the factors which are responsible for this improvement.

For the sake of simplicity we have have spoken as if response patterns are totally instinctive or totally learned. We have no reason to suppose that this is the case. It seems more probable that instinctive elements and learned elements contribute jointly to the formation of a single response. For instance, when our subjects were first placed on their feet, they did not support themselves but they did give an occasional push with the legs. These extensor thrusts in response to pressure on the soles may have been unlearned. In later trials, extension of the legs became more persistent, more regularized, and more energetic so as finally to lead to support of the body. We have said that the response was learned. By this we mean that some learning was undoubtedly involved, since the response did not appear at the usual age when opportunities for practice had been eliminated. However, this does not involve a denial that instinctive contributions may also have been present. Instinctive elements by themselves did not lead to "standing with support" but they may have been essential to the formation of this pattern. In any infantile pattern, instinctive bases and learned modifications may be inextricably mixed. Our aim has been to show that in all likelihood instinctive elements by themselves do not constitute normal infant responses.

Figure 5. Rey (left) and Del (right) at four years of age.

SUMMARY

In this report the experimental conditions surrounding the development of our two subjects have been presented in some detail. The aim of the experiment was to throw the subjects as much as possible upon their own resources and their own initiative in so far as behavioral development was concerned. The experiment covered a period of 14 lunar months, extending from the end of the first to the end of the 15th lunar months.

These data have been interpreted as indicating two general conclusions.

1. The first of these is that practically all of the common responses of the first year of life may be developed autogenously. That is, infants will develop these responses without encouragement or instruction, without reward or example. It follows that prior to the second year of life sociogenous responses, those which are learned through the intercession of other persons, are few and are relatively unimportant. If the well-being of the infant is assured, his behavioral development will take its normal course.

2. The second conclusion is that in the development of the autogenous responses of the first year learning plays an important part. The dichotomy of learned and unlearned responses is difficult to employ, but if we are to apply these concepts the evidence indicates that there is little reason to believe that infant responses are made up exclusively of unlearned elements except in the case of very few reactions. However, responses which involve learning may at the same time involve instinctive contributions. As a corollary of this conclusion, it would follow that while maturation is a major factor in infant development its importance lies chiefly in making learning possible. Maturation in and of itself seldom produces new developmental items, but maturation of structures when accompanied by self-directed activity leads to new infant responses.

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Two—Relation of Infants' Weight and Body Build to Locomotor Development

JOHN GRAY PEATMAN &
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The research here reported shows conclusively that despite common opinion to the contrary, there is little or no relation between the relative weight or body build of an infant and the onset of standing and walking. Many such popular opinions prove to be wrong when subjected to scientific checks. How do such erroneous ideas arise? Fortunately, in this case, the scientist can suggest an answer. Apparently the belief that heavy children walk late is due to our perceiving as a cause of late walking a fact that is not a cause but only an accompanying circumstance. Because of his advanced age, a child who walks very late IS *large when he begins to walk, hence it is easy to attribute his lateness to his size. But he is no larger than children of the same age who are already walking. Nor was he any larger than other children at nine months of age when none of them was yet able to walk alone.*

INTRODUCTION

This investigation was initiated as a result of an interest in the hypothesis that heavier types of children tend to be retarded in the development of sitting, standing, or walking. At the inception of our inquiry, we had supposed that there was a good deal of evidence to support this hypothesis. The contention that heavier types of children are apt to walk later than lighter ones is evidently something more than a popular belief circulating among lay members of our culture. This is indicated by several current works concerned with child development, although scientific evidence for the hypothesis is not cited and does not appear to be especially available.

Thus, Louttit (2) writes that "the age at which these various steps [in the development of motor behavior] appear is affected to some extent by the weight of the child. . . ." No evidence for this statement is specifically cited; however, the author mentions Dr. Shirley's research in a sentence preceding the one just quoted. Referring to Shirley's intensive study of 25 infants, we find she suggests that retardation in motor development of two or three of her subjects may have resulted from their large size (5). Later in the same volume she writes, "In general it may be stated that thin, muscular babies and small-boned babies walk earlier than short, rotund babies and exceedingly heavy babies. But in the case of weight it is impossible to decide which is the cart and which the horse." Dr. Strang (6) also writes that "there is some evidence that the roly-poly, heavy baby walks later than the slender-built child," but no evidence is cited. She adds, "but body build has not been shown to be closely related to motor abilities."

In view of these professional conceptions, as well as the popular lay belief, we were interested in ascertaining the possible extent of locomotor retardation among the heavier babies of our group of infants. However, as will be indicated by the analysis of our data, we find no evidence to support the hypothesis under consideration. The heavier types of infants in our study do *not* tend to sit, stand, or walk at a later age than the lighter ones. *

SUBJECTS AND PROCEDURE

The data for this investigation were derived from 349 infants reared under conditions of relatively optimal pediatric and home care. The environment of these children and the type of pediatric care they received have been described in detail by us in an article on growth norms (3). Briefly, the 349 infants lived in the Towns of

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* Since the preparation of this article, a relevant article by Dennis has been published (1). He analyzed the onset of walking in 25 puberty praecox cases and found the average and range of their walking ages to be entirely normal. Thus there appears to be no retardation in this behavioral function despite the greater maturity of their skeletal systems and their proximity to adult dimensions and proportions. The 25 cases studied were obtained from an analysis of available writings on children attaining puberty during the first two years of life and with no record of pathology or of any conditions known to affect the onset of walking. Walking age data were reported by mothers. The distribution of ages ranged from nine to eighteen months, with an average of approximately thirteen and a half months.

Rye, New York, and Greenwich, Connecticut, or nearby neighborhood—an area suburban to New York City. All were in private care of the pediatrician and from homes of at least middle-class standards of living, which means that practically all parents of these children could afford to buy necessary equipment and services for child care, as indicated by the pediatrician. The infants' conditions of pediatric and home care were thus relatively optimal in at least such factors as preventive and therapeutic medicine, diet, sunshine, and exercise. Furthermore, the home environment was studied and necessary adjustments made, when possible, to insure ample rest and to prevent excessive or harmful stimulation.

All of the infants were full-term births. This is an important consideration in studies of infant development, where age differences are noted in months or weeks. Shirley suggests that the early retardation in locomotor development of one of her subjects might have been chiefly due to the fact that the girl was premature by about seven weeks (5).

All physical measurements of weight and height were made under standarized conditions. The behavioral observations of locomotor development were made according to procedures which have been described by us in an earlier article (4). The beginning ages of sitting, standing, and walking for these infants were based upon the pediatrician's personal observations. Mothers' testimony was not used. The following criteria were employed.

1. *Sitting:* Age data for sitting are reported in terms of the child's ability to support himself alone in that position for at least one minute. For this observation, the child was placed in a sitting position on the pediatrician's examination table.

2. *Standing:* Age data for standing are reported in terms of the child's ability to pull himself into a standing position. This observation usually occurred during the consultation period between the mother and pediatrician, at which time the child was allowed free play about the room. As aids in pulling himself into a standing position, the child usually used a piece of furniture with which the office is equipped, a chair, table, desk, and the like.

3. *Walking:* Age data for walking are in terms of the child's ability to walk a distance of at least six feet or more without help of any kind, either initially, or during the process. This observation usually occurred during the same situation as described for *standing*. In case the child did not manifest obvious ability to walk about the room independently of aid, he would usually be seated at one side of the room and then asked to walk to the examiner, six feet or more distant.

It is relevant to add that, as a routine aspect of the pediatric care received by these infants, mothers were urged to allow their children to take their own time in beginning to sit, stand, and walk. A let-the-child-initiate-it attitude was recommended. On the other hand, freedom for motor development was prescribed—the freedom of light clothing and of a play pen or play yard.

ANALYSIS OF DATA

With such data on physical growth and locomotor development thus available, we proceeded to our present analysis.

First, it may be well to indicate relationships between weight and motor development, which no doubt account, in part at least, for the popular belief that late walkers are heavier. These are the relationships between *absolute weight* at the time of beginning to sit, stand, and walk with *age* at the time (with sitting, standing, and walking ages). These relationships are given by the correlations of Table I. They are of course all positive and range in value from .37 to .59. It is extremely unlikely that any of these correlations diverge from zero correlation only by chance, since they all express statistically a relationship based on the fact that infants increase in weight as they grow older. Here the early sitters, standers, or walkers tend to be lighter because they are younger; the later sitters, standers, or walkers tend to be heavier because they are older. Obviously, these correlations of Table I do not offer any evidence one way or another for the hypothesis in question. Our problem is one of determining whether our early sitters or early walkers are lighter *for their age;* whether late sitters or late walkers are heavier for their age—whether they are the roly-poly or extremely heavy types of children.

TABLE I

CORRELATION OF ABSOLUTE WEIGHT AT TIME OF BEGINNING TO SIT, STAND, AND WALK WITH SITTING, STANDING, AND WALKING AGES

Groups	*Sitting Age*	*Standing Age*	*Walking Age*
BOYS: All	.52 (N = 128)	.59 (N = 99)	.55 (N = 112)
of "107 Group"[a]	.53 (N = 51)	.54 (N = 51)	.50 (N = 51)
GIRLS: All	.38 (N = 130)	.37 (N = 105)	.46 (N = 120)
of "107 Group"[a]	.37 (N = 56)	.44 (N = 56)	.44 (N = 56)

All of the correlations of this table diverge significantly from zero correlation; that is, the null hypothesis may be rejected with confidence at the 1 per cent level.

[a] The "107 Group" referred to in this table and in Table II represents a part of the whole sample. It consists of those infants (107 in all) for whom a complete set of sitting, standing, and walking data were available. (4, p. 90 ff.)

Analysis I: Relative Weight

Our first analysis which bears directly on the hypothesis in question was initiated by obtaining each infant's *relative weights at the time* he or she began to sit, stand, and walk. For these measures of relative weight we used standard score values, taking the ratio of the difference between a child's absolute weight and the mean weight for the given sex and age level to the standard deviation of the corresponding weight distribution; thus, $(X_W - M_W)/_{\sigma W}$. The group data—the means and standard deviations—were obtained from our large group of infants, including these 349 cases, used in developing the growth norms already cited (3). Children heavy for their age and sex group would of course have positive standard score values for relative weight; the lighter children would have negative values.

These measures of relative weight were then correlated with the children's sitting, standing, and walking age values, with the results presented in Table II. Although all of these correlations are negative, only two, marked with footnotes, can be considered as significantly greater than zero; that is, the standard errors for each group are, with these two exceptions, too large to warrant the rejection of the hypothesis that the coefficients obtained are but *chance* divergences from zero correlation. In the case of the boy infants, there appears to be an extra-chance tendency for those who are heavier for their age to begin to sit up earlier than those lighter for their age. This tendency is very noticeable on the correlation matrices and does not arise from a scattering of only a few cases. It is a group trend. *

TABLE II

CORRELATIONS OF RELATIVE WEIGHT AT TIME OF BEGINNING TO SIT, STAND, AND WALK WITH SITTING, STANDING, AND WALKING AGES

Groups	*Sitting Age*	*Standing Age*	*Walking Age*
Boys: All	—.34 [a] (128)	—.08 (99)	—.02 (112)
of "107 group"	—.34 [b] (51)	—.18 (51)	—.16 (51)
Girls: All	—.13 (130)	—.17 (105)	—.02 (120)
of "107 group"	—.04 (56)	—.12 (56)	—.06 (56)

The size of each group is indicated in the brackets following each correlation coefficient.

[a] Significant at the 5 per cent confidence level.

[b] Significant at the 1 per cent confidence level.

* The average sitting age of the 10 infants relatively the *lightest* in weight was approximately eight and a quarter months. The average sitting age of the 20 infants relatively *average* in weight was approximately seven months. The average sitting age of the 10 infants relatively the *heaviest* in weight was approximately six and a quarter months.

Although these zero order and negative correlation coefficients may not be conclusive evidence for the rejection of the hypothesis that heavier types of children tend to be retarded in the development of sitting, standing, and walking, certainly the results present no shred of evidence for its acceptance. If the implications of this hypothesis applied to our sample of infants, then all of the correlations of Table II should of course be significantly greater than zero and *positive*. Not only is this not the case, but we we find for one group a contradictory implication, that is, the relatively heavier boy infants tend to begin to sit alone at an earlier, rather than later age.

A possible shortcoming, however, of this analysis arises in the fact that there is far from a perfect correlation between weight and height, or between weight and type of body build. It is possible, for example, for a very short roly-poly type to weigh less than the average of his age group, or for a tall, thin child to weigh as much as the average of his age group. On the other hand, the large, heavy type of child, as well as the small, light child, is properly located from the mean of his group by the method used in this analysis of relative weight.

Analysis II: Weight and Indices of Body Build with Age Constant

We have therefore employed a second type of analysis—that of keeping age constant for the physical measures of weight and body build. We have correlated absolute weight at six months with sitting age; absolute weight at nine months with standing age; and absolute weight at twelve months with walking age. These respective age levels were chosen because each, in turn, is at about the age at which some of these children were beginning to sit, stand, and walk. Similarly Weight/Height and Weight/Height^3 ratios (as two indices of body build) for each of these three age levels were correlated with the appropriate developmental age values. The resulting correlations are presented in Table III.

Again all are of zero order except for the boy infants and sitting age. Here, two of the three correlations obtained are sufficiently high to indicate the likelihood of an extra-chance tendency. The correlation of the absolute weight of the boys at six months of age with their sitting ages (which ranged from five through eleven months) diverges negatively from zero correlation with significance at better than the 5 per cent confidence level. And the correlation of Weight/Height ratios, as indices of body build, with their sitting ages is significant at the 1 per cent confidence level. Since both W/H and W/H^3 ratios are large for small roly-poly types, as well as for large, heavy types, a negative correlation with sitting age means again that there is a tendency for the heavier type of child to sit up earlier than the lighter type. It is to be emphasized that this trend is too slight to have any significance for individual prediction. We cannot validly infer that a heavier type of child will sit up earlier than a lighter type. The more important consideration is the negative character of these results for the original hypothesis in question. Again, by this second analysis, we find no evidence to support the view that heavier types of infants are later sitters, standers, or walkers than lighter ones.

TABLE III

WITH AGE CONSTANT: CORRELATIONS OF ABSOLUTE WEIGHT, AND WEIGHT/HEIGHT AND WEIGHT/HEIGHT ^{3} RATIOS OF BODY BUILD WITH SITTING, STANDING AND WALKING AGES

Physical Measurements at 3 Age Levels	*Sitting Age*		*Standing Age*		*Walking Age*	
	BOYS	GIRLS	BOYS	GIRLS	BOYS	GIRLS
N =	[100]	[108]	[75]	[88]	[97]	[100]
AT SIX MONTHS OF AGE						
Absolute Weight	−.21[a]	−.13				
Weight/Height Ratio	−.28[b]	−.12				
Weight/Height ^{3} Ratio	−.18	−.08				
AT NINE MONTHS OF AGE						
Absolute Weight			.04	−.01		
Weight/Height Ratio			.08	−.03		
Weight/Height ^{3} Ratio			−.03	.05		
AT TWELVE MONTHS OF AGE						
Absolute Weight					−.02	−.18
Weight/Height Ratio					.07	−.17
Weight/Height ^{3} Ratio					.13	−.07

[a] Significant at the 5 per cent confidence level.
[b] Significant at the 1 per cent confidence level.

It is, however, apropos to consider very briefly the reasonableness of the negative correlation for the sitting ages of the boy infants. At the time of the development of sitting behavior, the head comprises a considerable proportion of the total body weight or volume. The weight distribution of added pounds is generally below the head region. Consequently, there may be a sufficient lowering of the center of gravity of heavier types of boys to have a real, although slight, effect on the beginning of their sitting behavior. The heavier type of boy no doubt has more to sit on once he gets into a sitting position. The development of sitting equilibrium may thus be somewhat facilitated.

The negative correlations in the case of the girl infants are not as marked as those for the boy infants. Statistically, they are not large enough to warrant the rejection with confidence of the null hypothesis. This result suggests the possibility of a slight sex difference, perhaps arising out of the difference in the body build of boy and girl infants.

DISCUSSION

In concluding, we wish to emphasize what may be the obvious, that generally the real reasons for the age differences that exist in the beginnings of sitting, standing, and walking of infants are to be found in internal conditions and external circumstances having little or nothing to do with differences in weight or body build. It seems very likely that the popular belief in the retardation of motor development among heavier types of infants is nothing more than an old wife's tale. Such a belief provides a neat rationalization for the parents of those heavy children who turn out to be late sitters, standers, or walkers. Such a popular misconception is of course understandable. It has no doubt developed in response to definite parental needs, but unfortunately without benefit of the scientific procedure of recording exceptions to the supposed rule.

Once infants have developed the muscular and skeletal equipment for sitting, standing, or walking—and there are no doubt marked individual differences in this regard—the future course of their actual locomotor behavior will be characterized by the temporal variations typical of differences in learning processes. Some will learn quickly; some slowly. And the differences in the rate of such learning which are found to exist are no doubt mainly attributable to differences in temperament, in motor alertness and responsiveness to the relationship of internal needs and external circumstances. No doubt many children sit up or stand or walk at a later age than they otherwise might mainly because they lack the need or urge to try. Whatever the reasons, physiological or psychological, there seems to be no question that some infants are less curious, less persistent in the face of repeated failure, less alert to capitalize and consolidate a developmental gain, however slight. Prolonged illness or the emotional shock of a bad fall also may operate to retard locomotor development. But it appears most unlikely that excessive weight provides us with a significant clue to any of these conditions.

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Three—Influence of Illness During the First Two Years on Infant Development

STEVENSON SMITH

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Smith has demonstrated that certain childhood illnesses are accompanied by some degree, although not a great degree, of motor retardation. Smith warns that from his data one cannot conclude that these illnesses CAUSED *retardation. It may be that children susceptible to these diseases are slow in development, or that families who do not protect their children against pneumonia, diphtheria, and rickets also fail to provide other conditions that are optimal for development.*

The student should bear in mind that although the effect, if any, of these diseases is small, they are not diseases of the nervous system. It is well established by other studies that direct injury to the nervous system incurred by head injuries at birth, by hydrocephalus, or by infections and inflammations of the brain may have a profound effect, even to the point of producing idiocy.

It is a matter of common belief that sick children are slower to develop than are well children. In fact, this is statistically demonstrable. But here we encounter a danger of false interpretation. The methodologist will at once point out that, although sick children are slow to develop, this slowness may with equal right be charged against their constitutional weakness or attributed to some specific illness. It is the analysis of these factors that will concern us.

This study deals with the age at which children, *according to their parents*, hold up their heads, sit up without back support, cut the first tooth, creep, walk alone, say words in response to seen objects, and put two or more words together into a phrase. This information was secured concerning 94 per cent of the pupils of a Seattle grade school (912 cases). Some of the records were incomplete, but there was little, if any, selective sampling.

The trustworthiness of parents' testimony concerning the age at which their children develop the traits in question is none too great. A constant error would, however, have no effect upon our conclusions. Variable errors tend toward compensation and probably have little effect upon mean scores. The greatest source of selective error in this study would be a possible rationalization on the part of parents of slow children by which they seek to excuse their children's backwardness. This might conduce to an exaggeration of the remembered illness. But most of the illness reported here were distinctly of an all-or-none sort.

Eighty-one cases, whose histories were complete, gave a history of severe illness during the first twenty-four months of life. The others denied such a history. By severe illness is meant any of the five "children's diseases," pneumonia, diphtheria, any dangerous gastro-intestinal malady, rickets, a major operation, or a head operation.

A comparison of the mean ages at which the sick group and the well, or control, group develop each of the seven traits that have been mentioned would be of but little interest. This is because among the sick group some of the children had developed any particular trait before illness and some after illness. The mean age of developing the trait might be increased only by those who had previously been ill.

Again, if we calculate separately the mean age of developing a certain trait by those of the "sick" group who acquired the trait *before* their illness came upon them, and compare it with the mean for those who developed the same trait *after* their illness, we have results that are wholly misleading. One reason that those who develop the trait before illness do so earlier than do those who develop it after illness is that the two groups are selected. The naturally early developers will have acquired more traits before sickness overtakes them than will the late developers. Therefore, when classified with reference to the acquirement of any trait, there will be a preponderance of early developers among those acquiring the trait before illness and a corresponding preponderance of late developers among those acquiring the trait after illness. The direct effect of sickness in retarding development, as well as the effect, if any, of precocity or innate slowness in rendering the individual liable to disease, is not indicated by such measurements if they are carried no further.

The percentage of children that acquire any trait after illness has overtaken them depends upon the age at which that trait is developed. For a trait developed early the percentage will be relatively small. For a trait developed late it will be relatively large. It would follow, then, that the relative difference between the developmental age means of the sick group and of the control group would increase from trait to trait directly with the lateness of trait development, provided that illness causes the retardation. In order to make comparison possible, the corresponding means of the sick group and of the control group are expressed as ratios. Also the percentage is given of the sick group that developed the trait after illness (Table I).

TABLE I

Trait		*Mean Age in Months*	*S.D.*	*N*	(*x*) *Per cent Developing Trait After Illness*	(*y*) *Per cent of Retardation* $\frac{M_s - M_c}{M_s}$
Head up	control	2,400	1.31	513	25	.024
	sick	2.460	1.43	81		
Sitting up	control	5.341	1.49	619	49	.076
	sick	5.778	1.81	81		
First tooth	control	6.965	2.48	742	56	.066
	sick	7.454	2.86	81		
Creeping	control	8.440	1.75	583	71	.099
	sick	9.372	2.26	81		
Words	control	10.261	3.90	602	74	.078
	sick	11.126	3.85	81		
Walking	control	12.772	2.23	831	91	.113
	sick	14.403	3.82	81		
Phrases	control	14.487	4.39	503	97	.119
	sick	16.450	4.21	81		

The influence of illness will be shown as the quotients in column y vary with the percentages in column x. The correspondence is high (Figure 1). We may say, then, that there is a decided relationship between illness and retardation of development. The truth of this does not have any bearing upon the presence or absence of other factors. It does not eliminate, for example, the possibility that a third factor, such as some constitutional weakness, is responsible for both illness and retardation. The answer to this doubt lies, however, within our data.

If constitutional weakness alone had been the cause of retardation the correlation of x and y would have been 0, all the ratios in the y column being then presumably the same. There might, of course, be a perfect correspondence between illness and retardation and a perfect correspondence as well between constitutional weakness and retardation. But if constitutional weakness causes retardation, and is more frequently present in the sick group that in the well group, x will equal $ay+b$, were b is a constant representing the effect of constitutional weakness (Figure 1). This, however, is hardly the case according to what seems to be the line of best fit. If there is such a thing as constitutional weakness that renders babies liable to disease its association with backwardness of development is not indicated by these results.

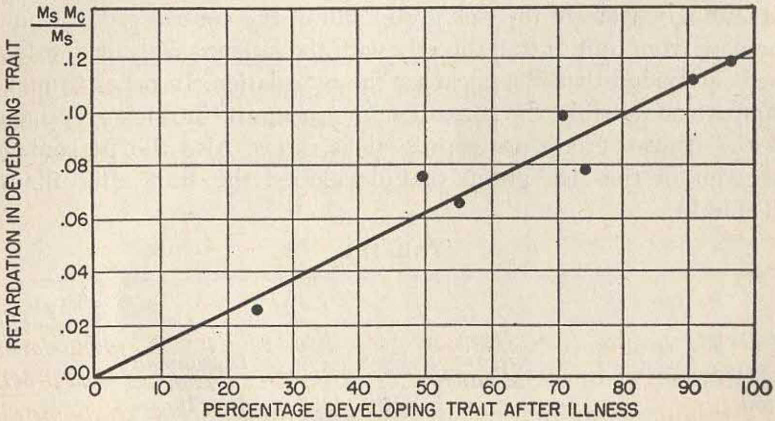

Figure 1.

Any conclusion drawn from these data will be no more reliable than the data themselves. The study should be repeated, using direct observation of babies and contemporary records.

By inspection of Figure 1, we would conclude that severe illness preceding the development of any trait reduces the apparent rate of development of that trait to about 88 per cent of what it would otherwise have been. Or, stated differently, the most probable amount by which the age of developing a trait will be increased by severe illness is 14 per cent.

Four—Influence of Cortical Development Upon Early Behavior Patterns

MYRTLE MC GRAW

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We are likely to think of growth as implying a progressive improvement in coordination or strength. The contribution of the following studies by McGraw lies in showing that certain early coordinations of infants, specifically suspending the body by the hands and engaging in swimming-like movements, actually deteriorate with age. Presumably this deterioration is due to the growth of the cerebral cortex, which inhibits some of the activities of the lower brain centers.

SUSPENSION GRASP BEHAVIOR

The ability to suspend the body weight by the force of hand grip on a rod is an infantile reaction which lends itself readily to numerical determinations. Simple stop-watch determinations of the time suspended provide values adequate to demonstrate the general course of development. This method was used in making 5,138 determinations on a group of children over a period of several years. The data were averaged over 10- or 20-day intervals during the first 300 days and over 200-day intervals thereafter. The trend of development during the first 300 days is shown by curves A in Figure 1. From these curves it is clear that the intensity of response tends to increase during the first 30 days, after which there is a gradual decline. Single-hand suspension is suppressed. Curves B in Figure 1 illustrate the course of development after the first 300 days. In general, suspension time is steadily increased. It might be said that the first 30 days represent an expansion in the development of nuclear centers governing this function. The subsequent decline probably corresponds to the onset of some inhibitory influence of

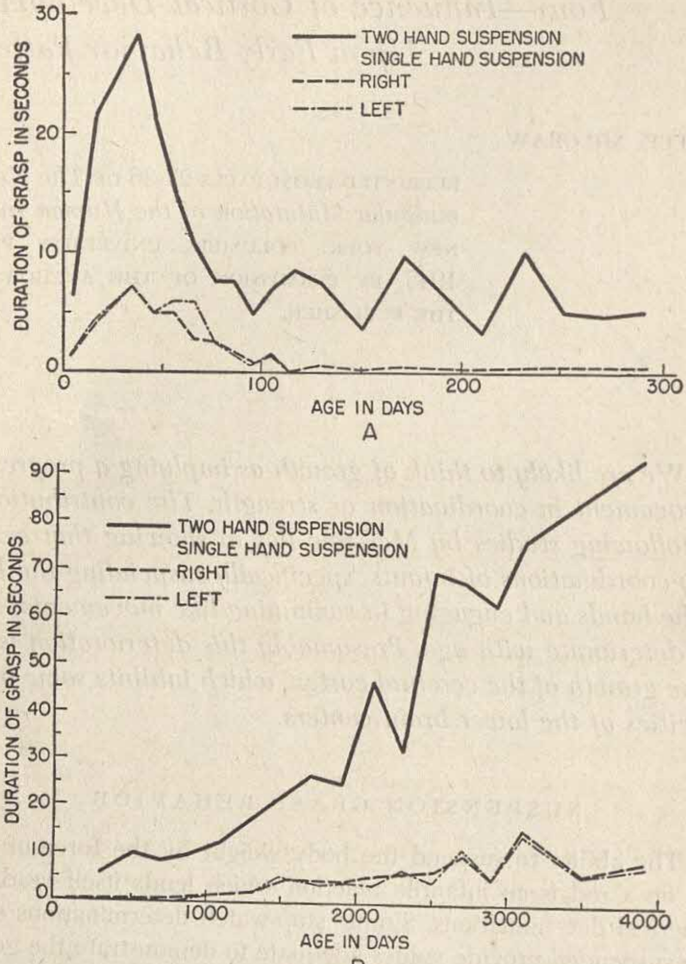

Figure 1. The suspension time for the group of children. (a) suspension time averaged over 10-day intervals during the first four months and over 20-day intervals thereafter; (b) suspension time averaged over 200-day intervals after the age of three hundred days.

cortical centers upon the activity of subcortical nuclei. Later, as illustrated in curves B, the ability recurs, but at this time under the dominance of the cerebral cortex. The general course of development is even more graphically illustrated by the logarithmic curve in Figure 2, which covers a period of eight years. The phenomenon manifests an increase in intensity during the first few weeks, followed by a period of progressive diminution in intensity of response. It reasserts itself as an activity under cortical control. The reflex phase is controlled by nuclear or subcortical centers; the decline or extinction of the powers of suspension is interpreted as an indica-

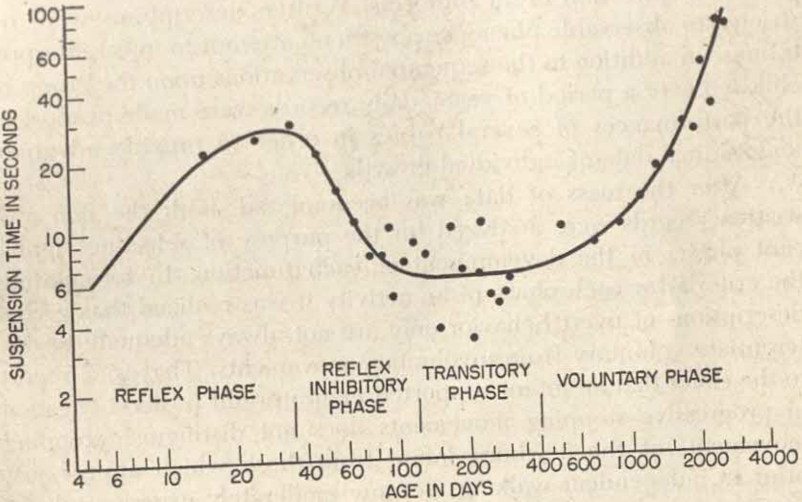

Figure 2. Suspension time for the group of children plotted on a logarithmic scale against chronological age, showing the trend of suspension behavior for all ages to eight years.

tion of development in cortical cells which exercise an inhibitory influence on subcortical functioning. The recurrence or reenforcement of the ability to suspend the body is regarded as evidence of maturation in those cells which activate or control neuromuscular movements and of their integration with neural centers which govern other factors involved in deliberate performances.

PROCEDURES WITH OTHER ACTIVITIES

Observations upon other neuromuscular functions of the infant were not so easily reduced to numerical values. However, observations were made and records were kept. It can be seen from the following accounts that the course of development of these more complex functions is not theoretically incompatible with the interpretations given to these simpler phenomena.

The procedure followed in studying other neuromuscular activities was fundamentally the same for each function. Observations were initiated immediately after birth of the baby or at the time of functional inception. Activities which were not a part of the newborn behavior repertoire could not be observed until some evidence of their emergence was indicated. Observations of each function in a number of children were made repeatedly until the activity attained a relative state of stability. Records were made on 16 mm. motion picture film or in protocols. Written descriptions were restricted to observable phenomena, with no attempt to inject interpretations. In addition to the sequential observations upon the group of children over a period of years, daily records were made of most of the performances of several babies in order to provide adequate longitudinal data of individual growth.

After the mass of data was accumulated, both the film and written records were analyzed for the purpose of selecting significant phases in the development of each function. In formulating the criteria for each phase of an activity it was realized that verbal descriptions of overt behavior only are not always adequate to differentiate voluntary from involuntary movements. That is, a report to the effect that an infant supported in an upright position engaged in progressive stepping movements does not distinguish stepping movements of the newborn infant from those which appear just prior to independent walking. Yet any moderately experienced observer can recognize the qualitative differences in such stepping. Furthermore, it was recognized that two babies may adopt movements which in pattern appear to be different but in fact reflect the same level of neural organization. For that reason a rationale based upon a theoretical interpretation of neural maturation was incorporated into the criteria formulated for appraising development in overt behavior.

Once the criteria were formulated for the phases of each function, it was possible to rate the many individual observations in accordance with developmental sequence. These ratings converted the descriptive data into a symbolic system so that the mass of data could be more easily manipulated. In making the ratings attention was focused upon those features common to the development of each function without specific regard for individual peculiarities. The method of rating was simple. A plus sign was ascribed to the phase most representative of the behavior as described or recorded on the film, and minus signs were assigned to other phases of the function. Because of the gradual process of growth a plus rating was on some occasions assigned to more than one phase in order to represent transitional periods more accurately. The number of phases in each function varied, as did also the number of observations, since some

PRONE PROGRESSION

	AGE IN DAYS		PHASE								
NAME	OBSERVATION	MOVIES	A	B	C	D	E	F	G	H	I
BRIGGS, JAMES		2	+	−	−	−	−	−	−	−	−
	11		+	−	−	−	−	−	−	−	−
	18		+	−	−	−	−	−	−	−	−
	24		+	−	−	−	−	−	−	−	−
		30	+	+	−	−	−	−	−	−	−
	38		+	−	−	−	−	−	−	−	−
	44		+	−	−	−	−	−	−	−	−
	52		+	−	−	−	−	−	−	−	−
		62	+	+	−	−	−	−	−	−	−
	65		+	+	−	−	−	−	−	−	−
	74		−	+	−	−	−	−	−	−	−
	81		−	+	−	−	−	−	−	−	−
	89		+	+	−	−	−	−	−	−	−
		95	−	+	+	−	−	−	−	−	−
	102		−	+	−	−	−	−	−	−	−
	107		−	−	+	−	−	−	−	−	−
	118		−	+	+	+	−	−	−	−	−
		124	−	−	+	−	−	−	−	−	−
	128		−	−	+	−	−	−	−	−	−
	135		−	−	+	−	−	−	−	−	−
	142		−	−	+	−	−	−	−	−	−
	153		−	−	+	−	+	−	−	−	−
		156	−	−	−	+	−	−	−	−	−
	167		−	−	+	−	+	−	−	−	−
	170		−	−	+	−	+	−	−	−	−
	177		−	−	+	−	+	−	−	−	−
		186	−	−	−	+	+	+	−	−	−
	191		−	−	−	+	+	−	+	−	−
	193		−	−	−	−	−	+	−	−	−
	199		−	−	−	+	+	−	+	−	−
		202	−	−	−	+	+	+	−	−	−
		205	−	−	−	−	−	+	−	−	−
	213		−	−	−	−	−	+	+	−	−
	220		−	−	−	−	−	+	+	+	−
		221	−	−	−	−	−	+	+	−	−
	227		−	−	−	−	−	−	−	+	−
		229	−	−	−	−	−	−	+	−	−
		230	−	−	−	−	−	−	−	+	−
	235		−	−	−	−	−	−	+	+	−
		237	−	−	−	−	−	−	−	+	−
		243	−	−	−	−	−	−	−	+	−
		250	−	−	−	−	−	−	−	+	−

Figure 3. Typical data sheet showing the ratings on one child in one activity during the first 250 days of life.

activities may attain relative stability within a few months, whereas the development of others extends over a period of years. A data sheet, after the ratings had been made on the observations of a particular child in a given function, is illustrated in Figure 3. Such a system of rating lends itself to the comparison of rates of development of different functions and also to an appraisal of an individual against a group of his contemporaries. However, our first concerns are the delineation of significant phases in the development of each function and a theoretical interpretation of the neural reorganization reflected in overt behavior. More specific correlation between structural and functional development must await further studies in neuroanatomy and neurophysiology; until these facts are available the organization of behavior data into a theoretical framework seems justifiable.

SWIMMING

After 445 observations of the aquatic behavior of 42 infants, ranging in age from eleven days to two and a half years, it became apparent that the significant developmental changes in such behavior could be conveniently classified into three phases, as follows:

Phase A, Reflex Swimming. When the newborn infant is submerged in a prone position the organization of neuromuscular activity is striking. The baby usually remains in the prone position; and definite rhythmical associated flexor-extensor movements in upper and lower extremities, together with a lateral flexion of the trunk corresponding to the flexor phase of the lower extremity, are usually manifested. These movements are ordinarily sufficiently forceful to propel the baby a short distance through the water. The movements involve the total musculature and are usually better organized than newborn crawling or stepping movements. Ingestion of water and coughing after removal from the water are less common among the reactions of the newborn than in older infants. Apparently some reflex still functions during the early weeks of life sufficiently to inhibit breathing efforts on the part of the submerged newborn infant. Line drawings A of Figure 4 illustrate the character and rhythmicity of reflex swimming movements. Since the cerebral cortex is not functioning appreciably at this time, it is reasonable to assume that these movements are under the control of subcortical nuclei.

Phase B, Disorganized Activity. After the first few months the rhythmicity and organization of pattern become somewhat dissipated when the infant is submerged in water. There follows a period when the baby tends to rotate from a prone to a dorsal position and movements of the extremities are of a struggling order. It may be that some awareness of the environment is expressed by the infant's bringing his hands to the water line on his face. Less control over the respiratory mechanisms is evidenced by coughing and the ingestion of water. Line drawings B of Figure 4 represent the character of movements commonly exhibited during this phase. This change in aquatic behavior seems to reflect the development of some neural mechanism which serves to disrupt the organization of reflex activity. During this period neither cortex nor subcortical nuclei function for the optimum benefit of the baby. It is a period of transition from reflex to a more voluntary type of activity.

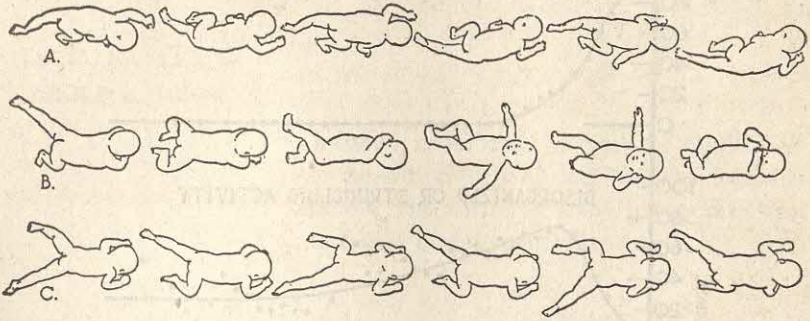

Figure 4. Three phases in the development of aquatic behavior of the human infant. (a) reflex swimming movements; (b) disorganized behavior; (c) voluntary or deliberate movements. These drawings were obtained by tracing successive frames of 16 mm. movie film illustrating the quality of consecutive movements made at different chronological or developmental stages.

Phase C, Deliberate or Voluntary Movements. At about the time when independent walking develops, another change is exhibited in the aquatic behavior of the child. Again the baby tends to remain submerged in the prone position, to engage in flexor-extensor movements of the extremities, especially the lower extremities, and to propel the body through the water. The quality of these movements differs from the rhythmical movements of the newborn; they are more deliberate and apparently voluntary. The child is not merely fighting; he is making purposeful movements, fairly well organized, but less automatic than the reflex movements, in order to reach the edge of the pool. The nature of neuromuscular movements of this order are illustrated by line drawings C of Figure 4. The quality of the movements and the child's awareness of his environment indicate that the cerebral cortex is participating in the activity. It is not until much later that the child gains the ability to raise his head above the water level in order to breathe. There is reason to believe that unless swimming experience is continued at this time, these deliberate movements characteristic of the ambulatory child will be abandoned as the horizon for sensory experience and judgment expands.

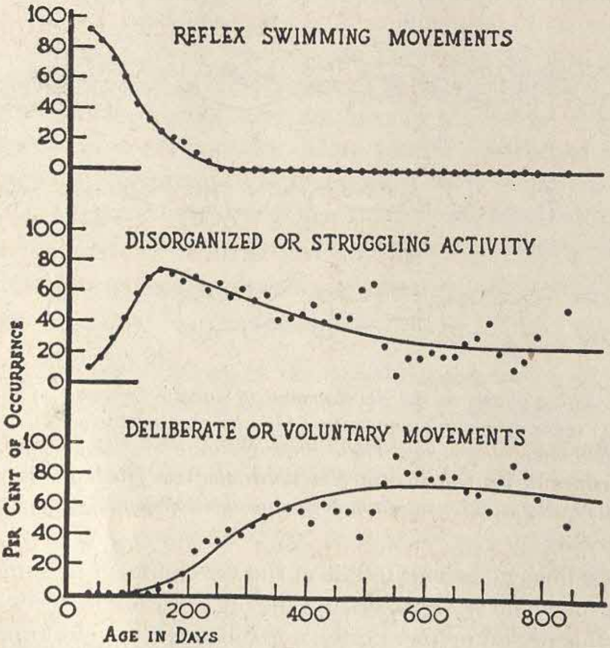

Figure 5. The incidence of three phases in the aquatic behavior of infants.

After the observations were rated in accordance with the above criteria, it was possible to calculate the developmental trend of each phase as demonstrated by the curves in Figure 5. Plus-minus ratings are too gross to demonstrate the rise in specificity of the reflex movements which occurs during the first three weeks or so of life. However, a greater variety of response becomes available as the cortex comes into play. To some extent it can be assumed that this variability of response is shown on the curve by the increase in scatter during the later age periods.

Five—Environmental Correlates of Mental and Motor Development

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Does the young child become more like his parents as he grows older? The following study shows that in regard to mental test scores, children show no resemblance to their parents before eighteen months but do develop a resemblance thereafter. Does this indicate an effect of the environment or does it merely indicate that tests given to children under eighteen months of age do not really measure intelligence? Perhaps the student would like to plan an investigation that would answer this question.

The development of methods for the testing of young-children has led to a number of studies dealing with the relationship of individual differences to factors in the environment. In the investigations of Goodenough (7), Van Alstyne (12), and others, the test scores of preschool children have been found to be positively correlated with parents' education and occupational ratings. Other studies, notably those of Furfey and his associates (5, 6), have revealed no relationship between these environmental factors and the scores of infants under one year. The present report provides a link between these discrepant findings, and has the further advantage of presenting cumulative results within a uniform, although small, sample. Its principal limitation is that of all studies which are primarily statistical-descriptive, and which lack either an experimental control of variables or an intimate analytic record of the developmental process.

THE SAMPLING

The group utilized in this study has figured in a series of previous reports dealing with various aspects of development (1), (2), (3). The purpose of the main investigation, as well as the practical requirements of a study involving long-continued cooperation from parents, determined to some extent the nature of the sample which was selected. Only hospital babies were used, due to the fact that the program required information concerning the mother's pregnancy and confinement, and also involved tests of the infants during the neonatal period. The sources were two hospitals drawing their clientele from different socio-economic levels. One is a county hospital including charity cases, while the other is a private hospital where cases from all income levels may be found.

TABLE I

FREQUENCY DISTRIBUTION OF STANFORD-BINET IQ'S AT 72 MONTHS

IQ	n 48	Per cent 100.0
170-179		
160-169		
150-159	1	2.1
140-149	5	10.4
130-139	10	20.8
120-129	13	27.1
110-119	12	25.0
100-109	5	10.4
90- 99	1	2.1
80- 89	–	–
70- 79	–	–
60- 69	1	2.1
Mean	123.3	
Median	123.0	
S. D.	15.6	

A pediatrician (Dr. L. V. Wolff) visited the hospitals and interviewed the mothers of all newborn infants who were normal at birth and had normal delivery. During the period between September 27, 1928, and May 15, 1929, 75 mothers were interviewed. In order to gain greater homogeneity of sampling and to simplify the task of securing information from the parents, cases were restricted to families in which both parents were white and English-speaking. They were also restricted to those who had established more or less permanent residence in Berkeley, and who could be expected to be available for cumulative study over a period of years. Of the 75 families interviewed, 59 proved to be cooperative and to meet the criteria for inclusion. The 59 families included two sets of twins, making 61 children in all, 31 boys and 30 girls. Six of the children were born in the county hospital, 55 in the private hospital.

Approximately one-half of the fathers and one-third of the mothers are college graduates. Approximately two-thirds of the fathers and three-quarters of the mothers are high school graduates.

THE MENTAL AND MOTOR TEST DATA

The program of mental testing has been as follows: From birth through fifteen months the children were tested at one-month intervals with the California First-Year Mental Scale (1). From fifteen through sixty months they were tested (first at three- and later at six-month intervals) on the California Preschool Scale I (8). At sixty-six months they were given a vocubulary test from the Thorndike CAVD and a series of form boards. At seventy-two months they were given the Stanford-Binet. At all visits, from one through seventy-two months, they have been scored on a series of motor tests. The motor tests for the first three years have been standarized and published under the title of "The California Infant Scale of Motor Development" (University of California Press, Berkeley, California); similar tests, involving both gross physical abilities and fine motor coordinations have been adapted for use at later ages.

TABLE II

COEFFICIENT OF RELIABILITY OF MENTAL AND MOTOR TESTS FOR THE FIRST SIX YEARS

(Split-half correlations with Spearman-Brown correction)

Age in Months	*No. of Cases*	*r for Mental Tests*	*r for Motor Tests*
1+2+3	52	.84	.76
4+5+6	54	.95	.82
7+8+9	45	.94	.82
10+11+12	50	.89	.93
13+14+15	45	.85	.88
18	51	.93	.83
21	53	.83	.76
24	48	.80	.74
27	51	.88	.82
30	47	.89	.82
33	44	–*	.86
36	49	.84	.80
42	43	.92	.83
48	45	.94	.71
54	44	.96	.86
60	47	.95	.94
66	46	–*	.91
72	46	.81	.88

* No mental tests were given at 33 or 66 months.

The reliabilities of the mental and motor tests are given in Table II for ages up to six years. For the first 15 months the reliability of a single test is unsatisfactory; this difficulty has been overcome by combining the results for three successive tests.

THE ENVIRONMENTAL DATA

The environmental data utilized here were obtained by an experienced social investigator (Miss Frances Welch) who visited the homes and interviewed the parents (usually the mother) in the first month or two after the child's birth. The information obtained includes the years of schooling of the mother and the father, the father's occupation, his earnings, the total family income, and descriptions and ratings of the house exterior, living room, home furnishings and equipment, and the neighborhood. These ratings of home and neighborhood are based upon the Berkeley Social Rating Scale. *

TABLE III

INTERCORRELATIONS BETWEEN VARIOUS SOCIO-ECONOMIC MEASURES

	Income	*Mother's Education*	*Father's Education*	*Occupation*
Composite Social Rating......	.82	.37	.57	.69
Family Income		.24	.59	.70
Mother's Education			.56	.57
Father's Education				.80

The composite social rating is based on a sum of five 5-point ratings of house exterior, neighborhood, living room, family accommodations and special equipment. In order to obtain linear regression lines, correlations for income were based on auxiliary scores in terms of the following logarithmic scale values:

Scale	*Annual Income*	*Scale*	*Annual Income*
1	\$ 792 – \$ 937	9	\$3060 – \$3619
2	937 – 1109	10	3620 – 4389
3	1110 – 1309	11	4390 – 5079
4	1310 – 1559	12	5080 – 6009
5	1560 – 1839	13	6010 – 7129
6	1840 – 2179	14	7130 – 8439
7	2180 – 2589	15	8440 – 10,000
			Over 10,000

Occupation was scored in terms of the Taussig rating of Father's occupation.

Table III presents intercorrelations of the principal environmental measures. The data indicate a substantial degree of interrelationship among the various factors considered. Husbands and wives are assortatively mated with regard to educational level; higher educational level is associated with higher occupational status, higher income, and higher socio-economic rating as to home and neighborhood. The highest correlations are between income and social rating, income and occupation, and father's education and occupation. From these coefficients, of course no inferences can be drawn as to direction of causation.

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* Reliability, validity, and intercorrelations of the elements in this scale are presented elsewhere (11).

An additional factor to be considered in interpreting the interrelationship of socio-economic measures, as well as their relation to other variables, is the factor of chronological age. The age of the father is correlated .26 with the composite social rating, and .37 with family income. Age of mother is correlated .39 and .38, respectively, with these variables, and .79 with age of father. The mean age of father (at the birth of the child) was 31.8 years, with an S.D. of 6.5. The mean age of mother was 28.4, with an S.D. of 6.2.

The factors listed in Table III have been combined into a total socio-economic score. The weightings of the factors are, approximately: Composite social rating, 1/3; family income, 1/4; occupation of father, 1/5; midparent education, 1/6. *

THE RELATIONSHIPS OF SOCIO-ECONOMIC VARIABLES TO MENTAL AND MOTOR STATUS AT SUCCESSIVE AGES

The different measures of socio-economic status—parents' education, occupation, income, composite social rating, and the total socio-economic score—have been correlated with the developmental scores of the children for all ages tested. The correlations with mental development are given in Table IV. A study of this table shows that for the first 18 months all *r*'s are either close to zero or tend to be negative. After this age some of the environmental factors develop a positive relation to mental scores while others remain only slightly related. Although the differences between single *r*'s are often not significant, the consistency of the trends for successive ages indicates that, for this particular sample, certain differences do hold. After the relationships become positive the highest *r*'s are with the mother's education (see Figure 1), reaching .5 at two years. Similar correlations with father's education are not found until the age of five years. Figure 2 gives the *r*'s at successive ages for the different items used in the total socio-economic scale. Midparent education is seen to be most highly related to mental scores,

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* These weightings were obtained by (a) summing the social ratings, with weighting of 1; (b) computing logarithmic scale scores for income, with weighting of 1; (c) multiplying Taussig rating of father's occupation by 2; (d) summing years of schooling of father and mother, and dividing by 4.

TABLE IV

RELATION OF CHILD'S MENTAL SCORE TO DIFFERENT SOCIO-ECONOMIC MEASURES

Age in Months	*N*	*Number of Years Mothers' Education*	*Number of Years Fathers' Education*	*Mid-Parent Educ.*	*Fathers' Occupation*	*Family Income*	*Social Rating*	*Total S. E. Scale*
1+2+3	50	−.15	−.07	−.14	−.12	.12	.24	.12
4+5+6	50	−.23	−.26	−.29	−.26	−.02	.03	−.10
7+8+9	49	−.01	−.09	−.08	−.05	−.09	.06	−.04
10+11+12	47	.06	−.06	.02	.01	−.05	.07	.05
13+14+15	45	.03	−.11	−.01	−.09	−.03	.04	−.10
18	44	.12	−.10	.16	−.06	−.07	−.05	−.10
21	46	.37	.19	.29	.16	.04	.18	.15
24	41	.52	.39	.50	.35	.20	.29	.34
27	44	.47	.21	.41	.27	.10	.28	.25
30	41	.48	.33	.44	.25	.08	.19	.20
36	43	.46	.28	.47	.23	.00	.16	.04
42	37	.46	.26	.38	.30	.07	.15	.18
48	39	.50	.37	.50	.31	.11	.13	.22
54	38	.40	.39	.50	.43	.19	.12	.26
60	40	.48	.53	.58	.43	.27	.26	.36
72	42	.58	.50	.59	.38	.32	.29	.41

and family income least, with the total socio-economic score falling somewhere between in its relationship. There is, in this group, a trend toward increasing correlation between mental performance and the various socio-economic factors which have been compared; correlations with income and social rating reach a peak at about .3, and with occupation and with the total socio-economic score at about .4. It may be noted that the Stanford-Binet (at six years) shows on the average about the same degree of relationship to socio-

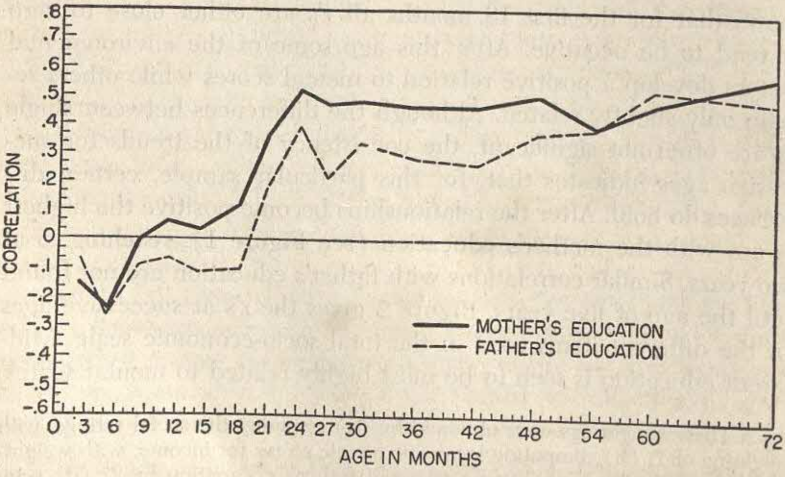

Figure 1. Correlations with mental scores.

economic variables as the California Preschool Mental Scale at five years.

Turning to the data for motor abilities, Table V indicates a much lower degree of relationship between socio-economic variables and this phase of development, than was found in the case of mental scores.* Correlations with mother's education, although low, are fairly consistently positive. Correlations with father's education, and with the family income are close to zero throughout. With the composite social rating and with the total socio-economic scale there is some suggestion of an increasing trend between 15 and 30 months, but none of the coefficients are significantly different from zero. The age of first walking and of first talking also fail to show any relation to the socio-economic variables considered here (Table VI).

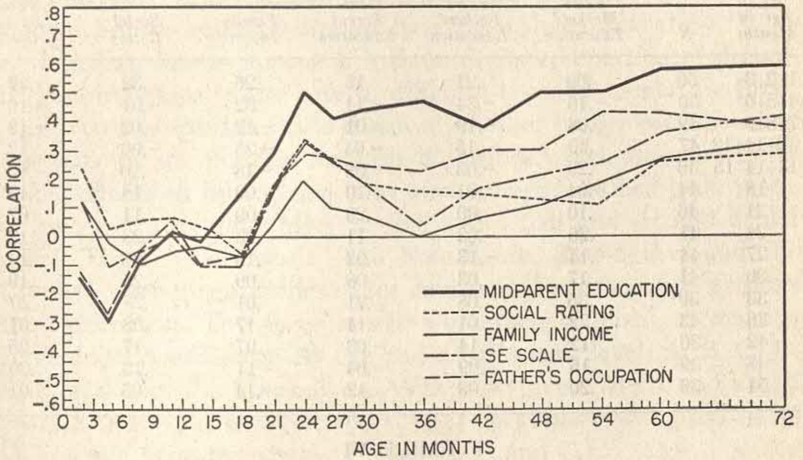

Figure 2. Correlations with mental scores.

As in the case of the mental scores, it is interesting to note the high incidence of slightly negative correlations with motor scores during the first year of life, suggesting the possibility of more rapid early mental and motor development among children from inferior socio-economic groups. If subsequent studies show that significance should be attached to these negative relationships, it would be interesting to explore two possible lines of explanation, (a) in terms of a biological hypothesis that within the human species as well as when human and infrahuman species are compared, precocity in infant development is associated with a lower limit of development, (b) in terms of an environmental hypothesis that children from economically inferior homes tend to experience a regime more stimulating to motor adaptive behavior, while children from superior homes are in infancy more sheltered and cared for and have less incentive for independent action.

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*Correlations after 54 months have not been computed with the motor scores as there was no evidence of directional trends in relationships.

TABLE V

RELATION OF CHILD'S MOTOR SCORE TO DIFFERENT SOCIO-ECONOMIC MEASURES

Age in Months	*N*	*Number of Years Mothers' Education*	*Number of Years Fathers' Education*	*Mid-Parent Education*	*Family Income*	*Social Rating*	*Total S. E. Scale*
1+2+3	50	.09	.21	.16	.26	.32	.27
4+5+6	50	−.16	−.34	−.14	.02	−.01	−.10
7+8+9	49	.08	−.19	−.01	−.12	−.02	−.12
10+11+12	47	.20	−.15	−.04	−.25	−.06	−.12
13+14+15	46	.29	−.03	.08	−.18	−.10	−.12
18	44	−.04	.01	.10	.09	.18	.12
21	46	.10	.00	.29	.09	.11	.07
24	41	.26	−.03	.11	.03	.23	.13
27	44	.14	−.13	−.02	.10	.28	.14
30	41	.17	.03	.08	.09	.25	.19
33	39	.35	.08	.21	.01	.28	.20
36	43	.22	−.01	.14	−.17	.08	.01
42	36	.12	−.14	−.03	.07	.17	.05
48	39	.15	−.09	−.04	−.11	.13	.00
54	39	.20	−.03	.12	−.14	.06	.01

TABLE VI

CORRELATIONS OF AGE AT FIRST TALKING AND WALKING WITH SOCIO-ECONOMIC VARIABLES

	Age of First Walking Alone		*Age of First Saying Two Words*	
	No. of Cases	*r*	No. of Cases	*r*
Mother's Education	55	−.09	53	−.22
Father's Education	55	+.03	53	−.19
Midparent Education	55	−.04	53	−.24
Family Income	48	+.15	46	+.01
Social Rating	55	−.03	53	−.05
Total S. E. Scale	53	+.08	51	−.13

Of the correlations presented in Tables IV, V, and VI, the highest are those between parents' education and the children's mental performance. In accounting for this relationship, consideration must of course be given to both educational and genetic factors. Similarly, the increase in correlations, from one to three years, may be due to the cumulative influence of educational factors, or to the delayed maturation of hereditary characteristics or to varying contributions of each. Our data cannot at present be utilized to distinguish between those nature-nurture variables, or to define their relative importance at successive ages. A possible clue to be followed in subsequent investigation may be found in the tendency for the child's mental scores to correlate higher with mother's education than with father's education. This cannot readily be explained on a genetic basis, but (in view of the greater association of the mother with the child) would be expected if the resemblance between parent and child is primarily nurtural. It is possible, of course, that the difference in correlation is a chance characteristic of this small sample. Similar results, however, have been shown elsewhere.*

Another line of approach is through the comparison of parent-child correlations in the case of different types of mental performance; the problem here is to discover whether higher coefficients of resemblance are found in the case of abilities which are more likely to be influenced by training. One such comparison has been made on this group for tests given at five and a half and six and a half years. Three form boards (the Four-figure, Two-figure, and the Casuist) were given at these ages, and scored according to Arthur's standardization. The three scores were then summed to make a single form board score. At five and a half years, levels A, B, C, D, E, F, and G of the Thorndike CAVD were given. At six and a half years a shortened series of the CAVD vocabulary was used—level D, and five items from each of levels E, F, and G.†

The form boards scores correlated .45 and .33 with midparent education at sixty-six and seventy-eight months, respectively; while the corresponding correlations for vocabulary are .33 and .39. A correlation coefficient (.41) has also been computed at six years between Stanford-Binet Vocabulary and midparent education. From these correlations, no case can be made out for a greater environmental influence on a verbal test than on a nonverbal test, although the latter would be expected to be less affected by social-cultural factors in the home.

ANALYSIS OF GROWTH CURVE DATA

A selection was made of two groups of children whose parents have the highest and the lowest education scores. Nine children are included from the high education category, and eight from

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*In an earlier study of the relationship of the mental test scores of parents and children, among children principally of school age, Jones (9) found slightly higher mother-child than father-child correlations. Both Goodenough (7) and Van Alstyne (12) report *r*'s for test scores of preschool children which are slightly higher with the mother's education, though in some instances the differences are very small.

† As the children were unable to read, it was necessary to modify the procedure with Levels E, F, and G, and administer these tests orally.

the low education group. Figures 3, 4, and 5 present a comparison of mental growth curves for children whose parents differ most in education. Each curve represents a child's scores, obtained by the Thurstone method of absolute scaling, for each age he was tested, from month one through month sixty. In these figures the compared curves are intersecting or close together during the first 12 or 18 months, after which the child from the superior group forges ahead. By way of contrast, Figure 6 presents the mean curves of five pairs of siblings who have been studied, and Figure 7 presents the means of 13 pairs of unrelated children who have been matched for midparent education. In these cases, the two sets of curves are prac-

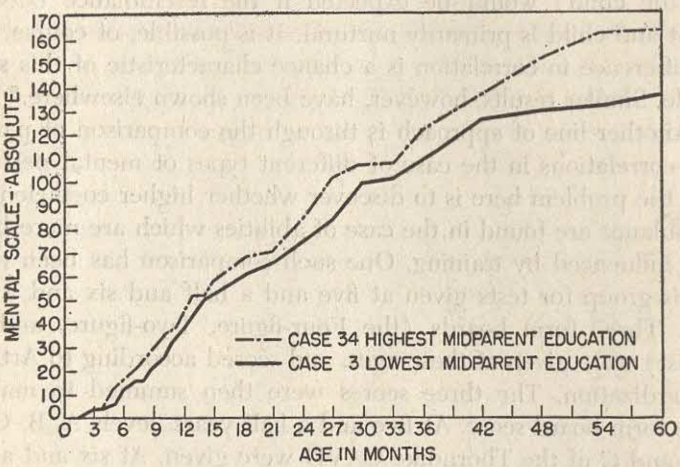

Figure 3. Individual curves, high and low education.

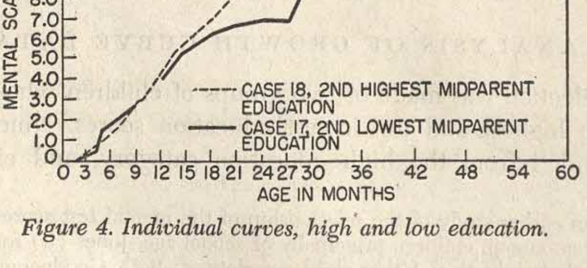

Figure 4. Individual curves, high and low education.

tically superimposed. The graphic data confirm the results of correlational analysis, in showing, after about eighteen months of age, an increasing correspondence between a child's mental scores and the social-cultural-economic position of his home. It is obvious, however, that neither the correlations nor the growth curves can do more than present a preliminary descriptive account of relationships. We must turn to other methods if an explanation of these relationships is required, and if we are to gain a more precise prediction and control over the phenomena involved.

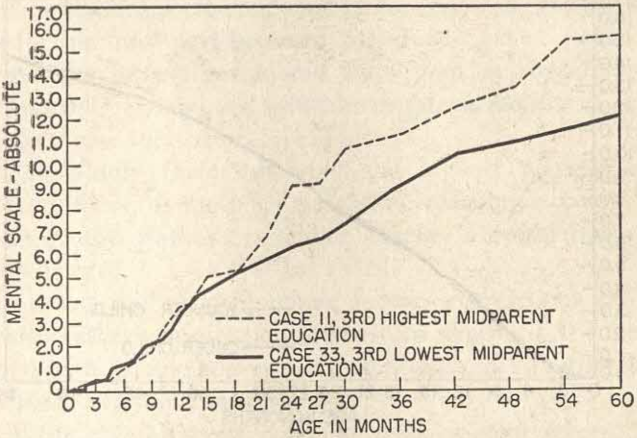

Figure 5. Individual curves, high and low education.

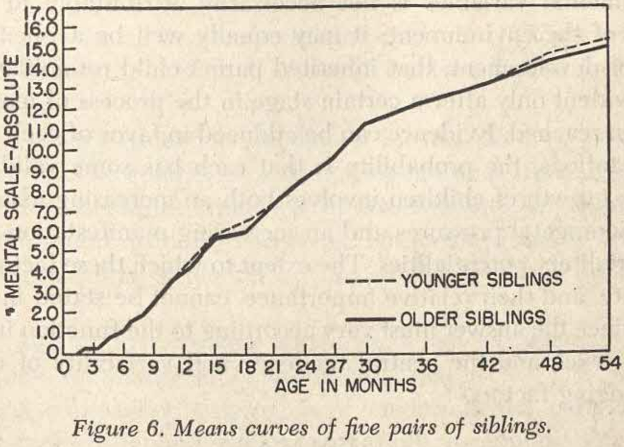

Figure 6. Means curves of five pairs of siblings.

From previous work, the hypothesis has emerged that test performance during the first 18 months is not diagnostic of intellectual ability. This is based partly on the greater community of function found between motor and mental scores in infancy (correlations are of the order of .5 during the first 15 months, after which the relationship drops markedly (2)). More directly pertinent is the fact that early mental test performance is uncorrelated with mental scores made after two years, even when a selection of the most "intellectual" items is used for comparison (2). This emphasizes the fact that the increasing correspondence between mental score and environmental variables is not necessarily attributable to the influence of the environment; it may equally well be a phenomenon of infant development, that inherited parent-child resemblances become evident only after a certain stage in the process of maturation has been reached. Evidence can be adduced in favor of each of these interpretations; the probability is that each has some validity, and that the growth of children involves both an increasing assimilation of environmental pressures and an increasing manifestation of complex hereditary potentialities. The extent to which these factors interpenetrate, and their relative importance, cannot be stated in general terms, since the answer must vary according to the function involved, the age level, and the central tendency and variability of each set of impinging factors.

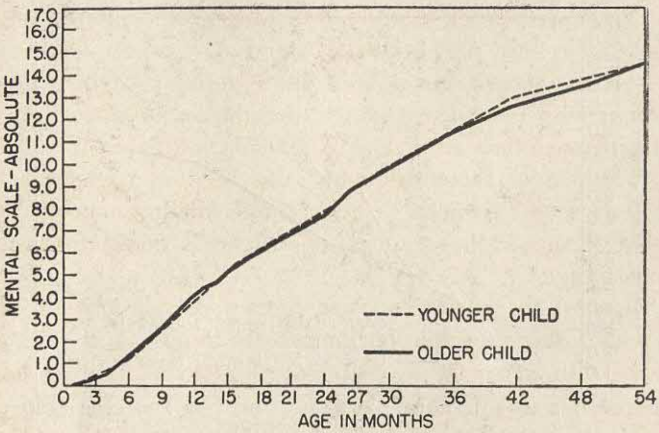

Figure 7. Mean curves of 13 unrelated pairs, matched for midparent education.

SUMMARY

1. In a group of 59 families in which children were born in 1928-29, a social investigator collected data on parents' education, family income, father's occupation, and social-cultural-economic factors represented in the Berkeley Social Rating Scale.

2. During infancy the children of this group were given the California First Year Mental Scale monthly to fifteen months; the California Preschool Scale I was administered 12 times from eighteen to sixty-six months, and the Stanford-Binet at six years. Reliability coefficients are presented for each age level.

3. The various socio-economic factors showed intercorrelations ranging for the most part between .5 and .8.

4. All the factors considered show zero or slightly negative correlations with mental test scores to eighteen months of age. Coefficients increase thereafter.

5. The single factor showing the highest correlation with mental tests scores is mother's education, reaching a peak of .5 at two years of age. Father's education reaches a correlation of .5 at five years of age.

6. Among the socio-economic factors correlations of mental scores with father's occupation increase to almost .4 at four and a half years; with income and with the composite social rating, correlations increase to about .3 at 6 years.

7. With a total socio-economic scale, mental scores show a correlation increasing from –.10 at eighteen months to .41 at six years.

8. A battery of motor tests was given at monthly, three-month, and 6-month intervals from one month of age to six years. Correlations with environmental factors are lower than in the case of mental scores.

9. Motor scores tend to correlate positively with mother's education, approaching a peak of about .3 between fifteen and thirty-three months.

10. Father's education and family income show no relation to motor scores. The composite social rating shows a slight relation to motor scores, increasing from –.10 at fifteen months to .28 at thirty-three months and declining thereafter. Although probable errors are high in this material, attention is called to age trends in the correlations.

11. Age of first walking alone, and age of first saying two words, show no relation to environmental factors.

12. A comparison was made of environmental correlates for different types of mental performance. Both vocabulary scores, and form board scores were shown to correlate about .4 with midparent education.

13. The correlational analysis for mental test scores is supported by individual growth data (mental growth curves in absolute

units) for children from homes of contrasting educational status. Theoretical implications are discussed.

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IV–THE EFFECTIVENESS OF EARLY TRAINING

One—Learning and Maturation in Preschool Children

JOSEPHINE R. HILGARD

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When a nursery school teacher works with her pupils on simple skills, she notes a considerable increase in proficiency over a period of weeks. She is likely to attribute this to nursery school training. What she does not know is the degree of improvement that a group not receiving special training would have shown in the same period of time. Even if the nursery school group is superior at the end of their training, the teacher does not know how quickly an untrained group would equal them once training is begun.

Hilgard's experiment is designed to provide the information that the teacher ordinarily does not have.

THE PROBLEM

The present experiment, which utilized a group of Merrill-Palmer nursery school children as subjects, was attempted in the hope that it might throw further light on the problem of maturation and learning in young children. The skills of buttoning, cutting with scissors, and climbing a ladder, which previous observations had shown to be developing in children between the ages of twenty-four and thirty-six months, were chosen for study.

Clues in regard to buttoning and cutting performances at this level appear in the Merrill-Palmer Scale of Mental Tests. According to this scale, the age at which the average child is first able to button a one-button strip is thirty and a half months; the age at which he can first button a two-button strip is thirty-three months; and the ability to cut gashes is an average performance for children from twenty-four to twenty-nine months of age. Though few children of this age are able to make a series of successive cuts in paper, there is so striking an improvement in this ability between the ages of thirty and thirty-five months that 32 per cent of 50 children of this age tested in the standardization of the scale were able to cut such strips. In contrast to buttoning and cutting, which involve the development of power and skill in fine motor coordinations, climbing brings into play gross motor coordination.

EXPERIMENTAL PROCEDURES AND MATERIALS

Two groups of 15 children each were matched according to chronological age, mental age, sex, and approximate initial ability in the three skills—buttoning, cutting with scissors, and climbing. Because of sickness, only 10 children in each group completed enough of the experiments to justify using their data. Table I gives the means for each group of children.

TABLE I

Group	*Mean CA*	*Mean MA*	*Sex*
Practice	28 3	29.1	6 girls, 4 boys
Control	28.6	29.9	6 girls, 4 boys

TABLE II

PRACTICE OF GROUPS, NUMBER OF PRACTICE PERIODS, INCLUDING RETESTS

Child	*Buttoning*	*Cutting*	*Climbing*
Kathryn	26	29	42
Mary E.	24	26	37
Marilyn J.	27	30	46
Jean	24	29	41
Cynthia	26	30	44
Frances	28	29	45
John	24	27	—
Patrick	25	25	38
Daniel	24	28	37
Stanley	27	29	36

Although the first tests were made chiefly in December, a few of the children were tested in November, and a few the first week in January. The mean initial test for both groups falls in December. Dating from the first week in January, the practice group received intensive training for 12 weeks. Table II shows the number of practice periods each child had.

After the first three weeks of training, the group was retested, and thereafter retests were made at two-week intervals when possible. At the end of the 12-week period the control group was retested and then received intensive training for four days. During this week of intensive training for the control group, the practice group received no specific training, but were tested at the beginning and again at the end of the training period.

In the following discussion of the techniques employed in testing the three skills, a description of the testing and retesting periods is in each case followed by a statement concerning the practice period.

Climbing

1. *Testing the skill attained in climbing.* This first test consisted of climbing a three-step ladder 2½ feet high, stepping on to a table at the top, and then climbing down again. On the table were various toys which might attract and please the child. These toys were changed frequently so that they did not lose novelty, and were varied to please individual children. Some of the boys, for instance, responded particularly well to engines, while other children liked toy animals. The usual formula was, "Let's see how quickly you can climb up on the table today," and "Now let's see how quickly you can climb down."

Going up the ladder, the child was timed from the second he put one foot on the first step until he had both knees or feet on top. Going down, he was timed from the second his knees were in the middle of the top of the ladder until both feet were on the floor again. The child's final score was obtained by averaging the time required for climbing up and that required for climbing down.

The pleasure the children experienced in the climbing test brought them into a cooperative frame of mind for the cutting and buttoning. They liked coming into the room to see what we had that was "new," and they particularly liked being on top of the table. After the child had examined the toy there, he was told he could hand it to the experimenter and she would put it on the little table (where he would next cut and button) while he climbed down. When this method was followed, the child would sit down at the little table ready to do the cutting and buttoning. Very often he was allowed to play with the toy for a short time before the next material was introduced. That the children retained their interest in the three tests is probably due very largely to this technique.

Only 8 of the 10 pairs of subjects completed this experiment. One child in the practice group was fitted with leg braces to straighten his knock-knees, and one in the control group did not take the initial test.

In plotting the learning curve for the practice group, it was necessary to interpolate to determine the score of the children at definite two-week intervals for, though no child had had less than six retests, these did not fall at the same time for all the children. For the control group, the curve was drawn without interpolation.

During the practice periods, the experimenter tried in every way to help the children eliminate wrong methods and improve the time and quality of their performance; for example, in training the child to climb faster, the experimenter showed him how to put one foot, instead of two, upon each step of the ladder. In other respects the practice periods were similar to the test period, except that the performance was not timed.

Buttoning

1. *Testing the skill attained in buttoning.* Usually the buttoning test was given immediately after the climbing test. In this test the child's performance was scored according to the degree of his success or the length of time he took to button strips offering different degrees of difficulty. The strip easiest to button was a folded piece of cloth with four buttons and corresponding buttonholes and a fifth button at the top already buttoned, so that the four buttonholes were in place over the buttons. Two of these buttons were ¾ of an inch in diameter and two ⅞ of an inch. If a child buttoned one button, he was given a score of one point; if he buttoned all four, he was given a score of two points. Performances on the other button strips have been standardized as a part of the Merrill-Palmer Scale of Mental Tests. The material consists of three pairs of 3 by 6 inch flannelette strips, the first pair having one button and buttonhole, the second pair two buttons and buttonholes, and the third pair four. All these buttons are ⅝ of an inch in diameter. That the child can button the four-button folded strip described above before he can manage the pair of strips having only one button is explained by the element of difficulty added when the two strips are separate and the buttonholes must be placed over the button before the child can button the strips.

The method of presenting the material to the child is practically identical with that used in the Merrill-Palmer Scale, and the reader is referred there for a full account. The folded four-button piece was presented first, and then, in turn, the one-button, two-button, and four-button strips. No assistance was ever given in a test situation.

Nine of the ten pairs of subjects were included in this test. The tenth pair was omitted from the results for several reasons. Though Bernice, the tenth child of the control group, matched Cynthia of the practice group in cutting and climbing, she scored 8 of the possible 13 points in the initial buttoning test, while Cynthia had a score of zero points. Further, when Bernice was retested in April, she scored 6 points on the test and could not score more than 7 points at any time. Since one atypical case, like that of Bernice, can noticeably affect the results when the number of cases is so small, and since the two children were never well matched in this skill, this pair was omitted.

The mean curve for the practice group was obtained by interpolation, as described in connection with the climbing test.

2. *Practice period.* During the practice period; the children had a wider variety of buttoning material from which to choose. A blue piece with two sizes of colored buttons down the sides made a very nice bag when all the buttons were buttoned. A red piece with black and white buttons in the form of a cross proved attractive to the children. There was also a blue folded piece with small colored buttons. During the practice period the child could select the ones he liked, though he was urged in various ways to make his choice from actual test materials. The experimenter assisted as much as was necessary and at the same time acceptable to the child. In practicing with the strips, for example, the younger children had to be coached on the method of getting the buttonhole exactly over the button.

Some of the children enjoyed buttoning; for others it was necessary to make the buttoning part of a game. One child who felt no fondness for buttons liked to put the buttoned strips into the baggage car of a toy train (secured in the climbing experiment) and send the train off on imaginary trips. Another child pretended they were blankets for the animals and after buttoning the strips put them on the animals to keep them warm. Though the interest in buttons was far from spontaneous toward the end, a fair degree of interest was maintained in ways such as these, varying much with the individual child.

Cutting

1. *Testing the skill attained in cutting.* The cutting test was given after the buttoning test. To secure a measure of the child's performance and progress in this test and to provide a basis for scoring, graph paper was used for the test material. A sheet of 8 by 10½ graph paper was cut vertically into two equal parts. On one of these were drawn two vertical red lines, 10 centimeters high and

parallel to each other; on the other a line rising to a height of 14 centimeters was drawn at an angle of 45 degrees to the base of the sheet. The child was given the sheet with the two vertical red lines and told to cut as carefully as possible along one of them, the experimenter indicating the point where he was to begin.

This cutting test held considerable interest for the children. Sometimes the parallel vertical lines on the graph paper were supposed, when connected, to form a door. After the child had cut along one of the lines, the experimenter would cut across the top to the other side and fold the piece back so that it looked like an open door and supported the paper. At other times, the cut paper was called a house, the experimenter cutting a "smokestack" from the top after the child had finished cutting. Other similar devices were used to hold the child's interest.

TABLE III

CUTTING: SCORING DEVICE

No manipulation	100 points
Opens scissors with both hands	95
Opens scissors with one hand	90
Cuts if paper is held for him	85
Cuts gashes. Holds paper himself	75
Cuts 3-3.9 centimeters	$\frac{{}^{*}D \quad 250}{{}^{*}L}$
Cuts 4-4.9 centimeters	$\frac{D \quad 200}{L}$
Cuts 5-5.9 centimeters	$\frac{D \quad 150}{L}$
Cuts 6-6.9 centimeters	$\frac{D \quad 100}{L}$
Cuts 7-7.9 centimeters	$\frac{D \quad 50}{L}$
Cuts 8-8.9 centimeters	$\frac{D \quad 20}{L}$

* D = Deviation; L = Length.

Since there is no test sufficiently similar to this to prove of assistance in scoring the child's performance, it was necessary to devise a scoring system. On the basis of the children's performances, a scale of successive levels was devised, ranging from 100 points for the poorest performance–no cutting at all–to zero for perfect adherence to the red-line length. Deviations from the red line were computed by counting the number of two-millimeter squares between the red line and the cutting. This total was then divided by the height attained. A system of weighting was soon found to be necessary, since it was obvious that the chances of any degree of deviation from the line were considerably less when the child could cut only 4 centimeters along the line than they were when he could cut the whole length of the line. The method of scoring is shown in Table III.

The data for all the 10 pairs of children were available in the cutting test. The learning curve for the practice group was obtained by interpolation, as in the other two tests.

2. *Practice period.* During the practice period the graph sheets just described and picture pages torn from magazines proved interesting as practice material. If the child was about twenty-four months of age, he had to be taught how to hold the scissors and coordinate the movements of the two hands; if a little older, how to make several successive gashes in the paper; and finally, he had to be taught how to guide the scissors along the lines.

RESULTS

At the end of the 13 weeks covered by the study, when the practice group had been trained for 12 weeks and the control group had been given a final week of intensive training, the outstanding result is a marked similarity in the gains made by the two groups. Though the practice group leads in the gain made in cutting and buttoning, the groups are practically the same in climbing. In the buttoning test the practice group gained 21.4 points and the control group 15.6, or 73 per cent as much. In cutting, the practice group gained 47.5 points and the control group 40.4, or 85 per cent as much. The difference between the two groups in climbing is slight and unreliable—9.2 for the practice and 9.6 for the control.

It is evident that the two groups were very evenly matched in all three initial skills, for only in cutting is there a difference of even slight reliability. In April, after the 12 weeks of intensive practice, there is a reliable difference between the two groups in all skills, and at the time of the final retest the practice group has a lead of low reliability over the control in cutting and buttoning, but the difference in climbing is negligible.

Certainly, the remarkable relative gains of the control group, with so limited an amount of training, suggest that factors other than specific training contributed to the development of these three skills. This is borne out by the fact that between the initial test and the first retest in April, before the week of specific training, the control group had gained 57.3 per cent of its total score in climbing, 72.5 per cent of its total in buttoning, and 82.4 per cent of its total in cutting. A comparison of the total gain made by the control group

TABLE IV

SUMMARY: PROGRESS OF THE PRACTICE AND CONTROL GROUPS IN CLIMBING, CUTTING, AND BUTTONING

	Initial Test December	*After 12 Weeks April*	*After 13 weeks April*	*Total Gains*
Buttoning				
(N = 9 in practice and control groups.)				
Practice group Mean ± *P.E.M.*	6.6±2.1	30.5±3.9	28.0±3.7	21.4±3.4
Control group Mean ± *P.E.M.*	7.7±2.1	19.0±2.6	23.3±3.1	15.6±2.2
Difference between means	1.1	11.5	4.7	5.8
P.E. of diff.	2.9	4.7	4.8	4.1
Diff./*P.E.* of diff.	0.38	2.47	0.98	1.41
Reliability of difference	Unreliable difference	Fairly reliable difference	Unreliable difference	Low reliability
Cutting				
(N = 10 in practice and control groups.)				
Practice group Mean ± *P.E.M.*	64.3±4.5	19.3±5.6	16.8±5.0	47.5±3.8
Control group Mean ± *P.E.M.*	73.6±4.4	40.3±8	33.2±7.2	40.4±4.8
Difference between means	9.3	21.0	16.4	7.1
P.E. of diff.	6.3	9.8	8.8	6.1
Diff./*P.E.* of diff.	1.48	2.14	1.86	1.25
Reliability of difference	Low reliability	Fairly reliable difference	Low reliability	Low reliability
Climbing				
(N = 8 in practice and control groups.)				
Practice group Mean ± *P.E.M.*	17.8±1.08	8.8±.95	8.6±.73	9.2±1.33
Control group Mean ± *P.E.M.*	18.4±1.76	12.9±.70	8.8±.75	9.6±1.11
Difference between means	0.6	4.1	0.2	0.4
P.E. of diff.	2.06	1.18	1.05	1.73
Diff./*P.E.* of diff.	0.29	3.47	0.19	0.23
Reliability of difference	Unreliable difference	Fairly reliable difference	Unreliable difference	Unreliable difference

with the gain made by this group during the one week of intensive training also throws some light on the nature of the factors involved. We find that in climbing 42.7 per cent of the total gain comes during the week; in buttoning, 27.5 per cent; and in cutting, 17.6 per cent. These gains are all considerably in advance of those made by the practice group during any one week of its 12-week training period.

In fact, the practice group in climbing required all of the previous month to make 42.7 per cent of its total points, a feat accomplished by the control group in one week. Thus, it would appear certain that the rate of learning was accelerated toward the close of the 13-week period covered by the investigation.

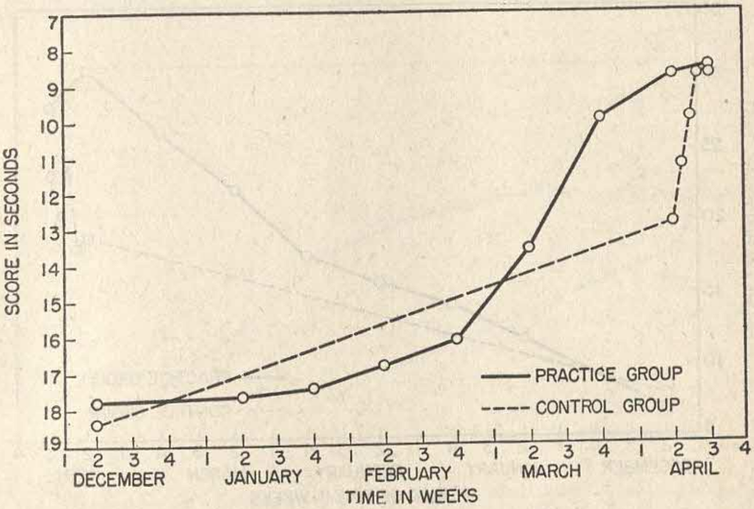

Figure 1. Mean learning curves for two groups in climbing (8 subjects in each group). Practice group trained for 12 weeks between the second week in January and the second week in April. Control group trained for one week only, between the second and third weeks in April.

The learning curves (Figure 1) for climbing are particularly significant in offering clues for the interpretation of the gains. An examination of the curves reveals positively accelerated learning for the practice group up to the last retest in March, learning being much more rapid toward the end of the period. On the supposition that the control group lags behind the practice group throughout (a fair assumption because it is behind on every test), it follows that the curve of improvement for the control group is also positively accelerated between the initial trial and the first retest, for without positive acceleration it would cut across the practice curve, as in the figure. This positive acceleration shared in by both practice and control groups we may interpret as signifying more favorable maturation and general developmental potentiality for climbing toward the end of the period. The practice group has taken a pronounced lead by the second week in April, showing that training effectively capitalizes this potentiality, and the very rapid improvement of the control group during the following week is further evidence in the same direction. That the two groups at the end are almost identical in their mean scores, the one after 12 weeks of practice, the other after one week of practice, shows the importance of the underlying factors. These curves therefore demonstrate more

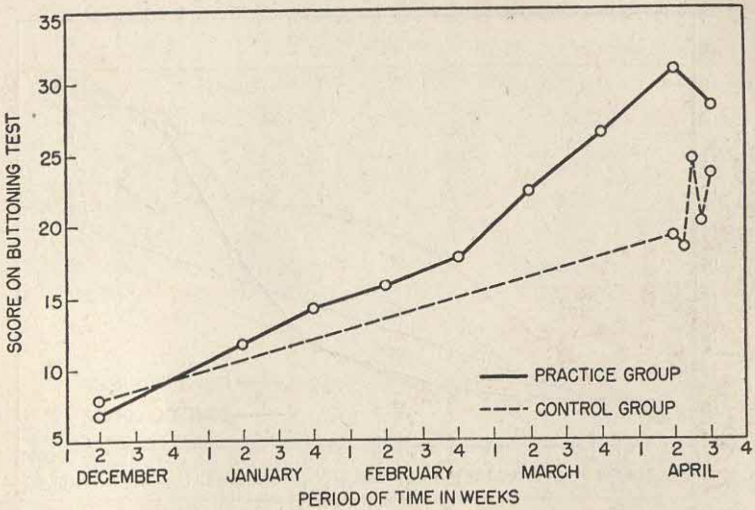

Figure 2. Mean learning curves for two groups in buttoning (9 subjects in each group). Practice group trained for 12 weeks between the second week in January and the second week in April. Control group trained for one week only, between the second and third weeks in April.

clearly than the others both the importance of the underlying factors and the temporary acceleration which can be introduced by special training. The decrease in acceleration of the practice group after the end of March suggests that there may be a limit to improvement during any given developmental stage, which is, of course, to be expected.

Where the curve for climbing shows only a slight gain during the first few weeks of training, the learning curve for buttoning rises steadily from the beginning, with a marked acceleration at about the middle of the training period continuing until the end of the period. During the thirteenth week, when the practice group had no training, this curve shows a decline of several points. Whether this drop is to be attributed to the discontinuance of training or to the fact that the children's interest in buttons had reached the saturation point is not clear. In view of the findings of Taylor and Gates, one may conclude that the gains made in a developing skill like buttoning as the result of special training are unstable in character and that the ability tends to resume its former level after a period of disuse. However, since the data for cutting and climbing do not show a similar drop after the end of the practice period, it may be that the length of the period of disuse necessary to bring about this drop in the level of ability achieved may vary both with the skill itself and with the degree of skill attained.

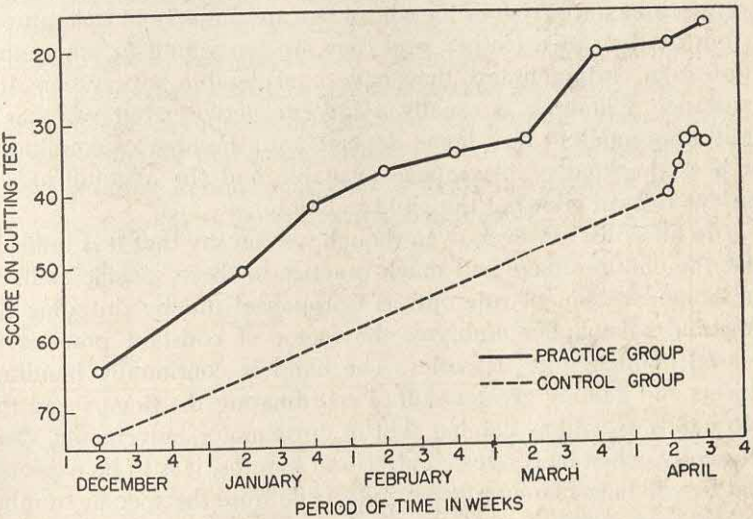

Figure 3. Mean learning curves for two groups in cutting (10 subjects in each group). Practice group trained for 12 weeks between the second week in January and the second week in April. Control group trained for one week only, between the second and third weeks in April.

A consideration of the data on cutting and buttoning yields some information concerning the values of these two items as part of a mental test battery. As was mentioned earlier, even though certain skills show age differentiation, they may not be desirable in such a battery if, at the same time, they are much affected by special training. If it had been possible to retest the practice and control groups at a later period the results bearing on this point would be more clear cut. Nevertheless, it is evident from the slight differences between the two groups at the end of the experiment that special training, though it had some effect, was far outweighed by the general developmental factors. The fact that the curve for buttoning had already started to decline lends credence to the view that the slight superiority attained in this skill may have been temporary.

In interpreting the findings of this experiment it must be remembered that the control group was given a retest and four training periods during the one week of intensive training, while the practice group averaged only two training periods a week during 12 weeks; consequently, the frequency of the practice periods may be one factor in determining the increase in the rate of learning evident at the close of the study. It is impossible, also, to eliminate the factor of specific practice in interpreting the gain made by the control group during the period of no specific training. Yet children from twenty-three to thirty-four months of age are unlikely to be required to button their own clothes, and they are too young to take much interest in cutting unless they have considerable supervision and assistance. Climbing is usually a favorite activity, but whether a child does much of it at home depends on a number of conditions, such as the kind of play space available and the attitude of the parents toward allowing the child to climb.

In all skills, however, even though we can say that it is unlikely that the children have had much practice in these specific skills in the home, we cannot rule out, as Carmichael did by drugging his frog and salamander embryos, the factor of constant practice in related manipulatory activities. The child is continually handling objects and gaining greater skill in coordinating the fingers and the two hands as well as gaining skill in gross motor movements. Consequently, when he is retested after four months, it is to be expected that he will have made progress quite aside from the specific training he has been given.

In this experiment, we cannot certainly distinguish between the gain to be attributed to maturation alone and that due to maturation plus practice in activities related to the specific skills studied. What does appear is that maturation, plus this related general practice, accounts for the great gain made between the initial test and the initial retest of the control group, and that specific training throughout the 12-week period was a far less important contributing factor in the development of these three abilities than was this general developmental trend.

SUMMARY

Two groups of 10 children each, aged twenty-four to thirty-six months, in the Merrill-Palmer Nursery School were equated for chronological age, mental age, sex, and approximate initial abilities in three skills: buttoning, cutting with scissors, and climbing. The practice group was given 12 weeks of practice, with retests at two-week intervals. The control group, without special practice in the meantime, was tested at the end of the 12-week period, and then given four days of intensive practice. The practice group and the control group were both tested again after this period. The initial differences between control group and practice group were unreliable on all the tests. After the 12 weeks of practice, the practice group exceeded the performance of the control group on all the tests, but one week of practice by the control group was sufficient to bring the scores of the control group and the practice group to similar levels. The rapid relative gains of the control group are interpreted to mean that factors other than specific training contributed to the development of these three skills, factors which may be partly accounted for by maturation and partly by general practice in related skills. There is evidence from the learning curves of the practice group in buttoning and climbing that improvement is more rapid in the latter part of the training period, consistent with the accelerated learning of the control group at the end of the experiment.

Two—Neural Maturation as Exemplified by the Achievement of Bladder Control

MYRTLE MCGRAW

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Is early training in bladder control effective? McGraw's approach to this problem was to begin the training of one of a pair of identical twins very early. The co-twin was given no training until a considerable degree of control has been achieved by his brother; the training of the co-twin was then begun. The experiment was repeated with a second set of identical twins. We shall let McGraw's results speak for themselves.

The objectives of the present investigation were (a) to determine the sequential changes in behavior which accompany and denote the development of voluntary control of micturition, and (b) to ascertain the effect of an early systematic training schedule upon the ultimate achievement of bladder control, that is, whether it accelerates, distorts, or is ineffectual in influencing the general course of development of this order.

Micturition is a convenient behavior to study as a basis for interpreting the development of neural organization because in the newborn infant it is a reflex activity presumably under the dominance of infracortical centers. As development proceeds, it assumes voluntary or deliberate qualities which undoubtedly reflect the influence of the cerebral cortex upon the reflex mechanism.

EXPERIMENTAL PROCEDURES

The subjects of the study were two sets of identical twin boys, Peter and Charles Dalton and Hugh and Hilton Putney, who were in the laboratory five days a week, between 9:00 A.M. and 5:00 P.M. The age range during which the Dalton twins were being studied

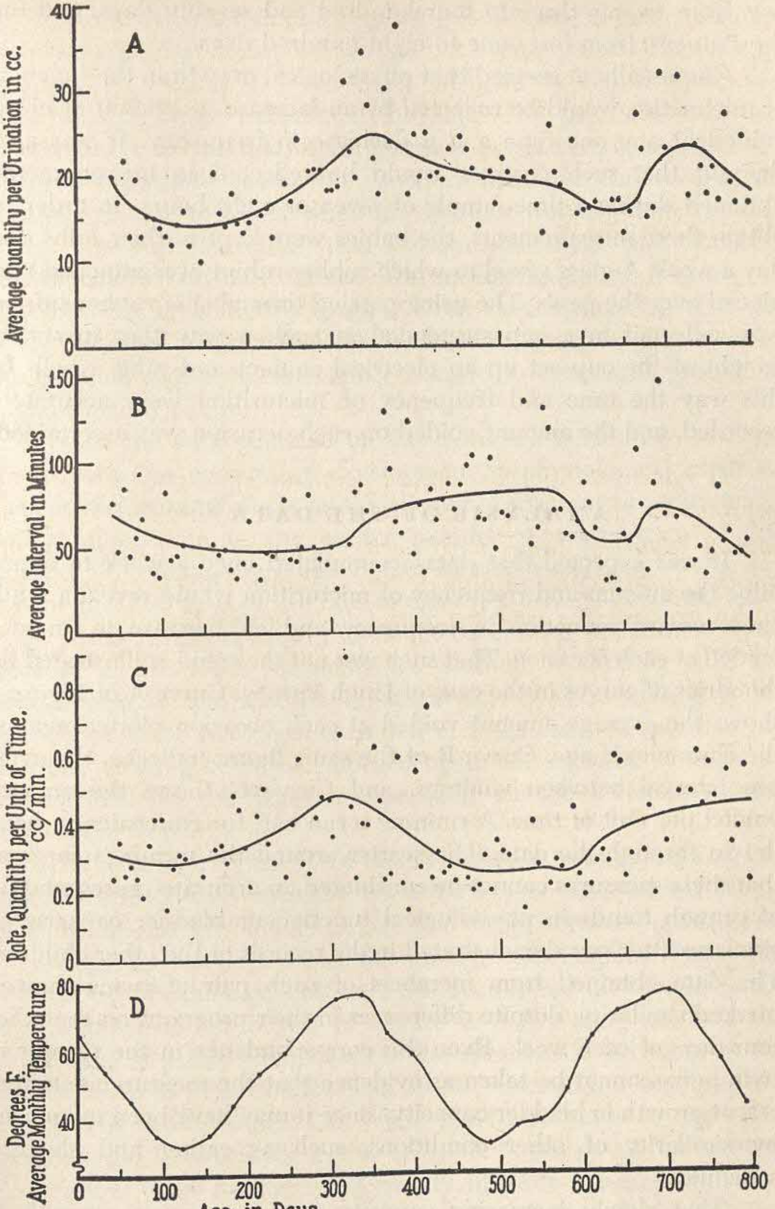

Figure 1. The first three curves show for Hugh Putney (A) the average amount voided on each occasion, (B) the interval between voidings, and (C) the rate or amount voided per unit of time, as obtained one day a week over a period of two years. Curve D indicates the average monthly temperature for the vicinity.

was from twenty-three to four hundred and seventy days, and for the Putneys, from forty-one to eight hundred days.

Empirically, it seemed that physiological growth in the function of micturition would be reflected by an increase in amount of urine voided at any one time and a decrease in frequency. It was also thought that such changes would be reflected in measurements obtained during a time-sample of seven or eight hours. In order to obtain these measurements, the babies were kept in their cribs one day a week. A glass vessel to which rubber tubing was attached was placed over the penis. The urine passing through the rubber tubing was collected in a cup suspended in such a way that increased weight of the cup set up an electrical contact and rang a bell. In this way the time and frequency of micturition were accurately recorded, and the amount voided on each occasion was ascertained.

ANALYSIS OF THE DATA

It was expected that data accumulated once a week to determine the amount and frequency of micturition would reveal a tendency toward reduction in frequency and an increase in amount voided at each occasion. That such was not the trend is illustrated by the series of curves in the case of Hugh Putney. Curve A of Figure 1 shows the average amount voided at each occasion plotted against the chronologic age. Curve B of the same figure indicates the average interval between voidings, and Curve C shows the amount voided per unit of time. A running mean has, for convenience, been drawn through the data. The scatter around the means is so great that these measures cannot be considered an accurate representation of growth trends in physiological function or bladder capacity. A similar scatter was demonstrated in the records of the other children. The data obtained from members of each pair of twins showed marked similarity, despite differences in their programs on the other four days of each week. Even this correspondence in the records of twin pairs cannot be taken as evidence that the measurements represent growth in bladder capacity, since it may have been influenced by similarity of other conditions, such as eating and sleeping schedules.

That slowly increasing capacity must accompany growth in anatomic size may almost be assumed as self-evident. That the influence of steady growth is not revealed clearly by the data in Figure 1 can only mean that other systematic causes of variability have operated to a degree which effectively obscures the influence of growth in size. Many possible causes of variability have been recognized and discussed in contemplating these data; in most instances accurate appraisal of importance has not been possible. The vicinity temperature (Curve D) has been included in Figure 1 as one determinable factor influencing the data. The parallel course between Curve D and the other curves through certain age periods suggests the wholly reasonable conclusion that the child was given a larger volume of liquid when the weather was hot than at other times. It seems, therefore, that 24-hour specimens plus accurate determinations of fluid intake are indispensable to a study of development in bladder capacity. Even then it may be that the whole neurophysiological mechanism is so complex that various components cannot be sufficiently isolated to determine growth in organ capacity per se.

While the data obtained on the twins are inadequate to draw any conclusions concerning development in physiological capacity, they do serve to cast doubt on the alleged functioning of a physiological rhythm, even in the earlier months. A scattergram of the number of voidings by Hugh Putney during each hour of the day for the first 300 days of life fails to exhibit a definite rhythmic tendency. Attention is drawn to this doubt concerning a rhythmic trend because so often the recommended method of training calls for conditioning of the basic physiological rhythm. The striking correspondence in the records of each pair of twins does indicate that the four-day training schedule imposed upon one member of each set did not influence the behavior appreciably on the days when urinal tubes were worn.

The data bearing upon the child's response to toilet training are more informative. A measure of achievement was calculated by plotting against chronological age the ratio between positive responses to the chamber and total number of urinations during the seven hours of each day. This ratio has been called the *success factor* and is shown in the curves of Figure 2. A declining trend in the percentage of positive responses to the chamber is indicated in the curves for both Peter Dalton and Hugh Putney after the first few weeks. The period when they were least responsive to the toilet situation was between the age of from six to ten or eleven months. From about three hundred and twenty to three hundred and sixty days there occurred a rapid increase in the success factor. The observations on the Dalton twins were terminated when they were four hundred and seventy days old, so that a decline following this initial spurt in improvement is suggested though not clearly demonstrated. That a decline was actually in progress is borne out by the curve of Hugh Putney. When the Dalton twins were four hundred and thirty days old, after Peter had begun to show a period of appreciable improvement, toilet training was initiated with his brother Charles. Even at the beginning Charles' success rating is only 10 per cent below that of his twin, who since the age of twenty-three days had been given the opportunity to associate the chamber with voiding.

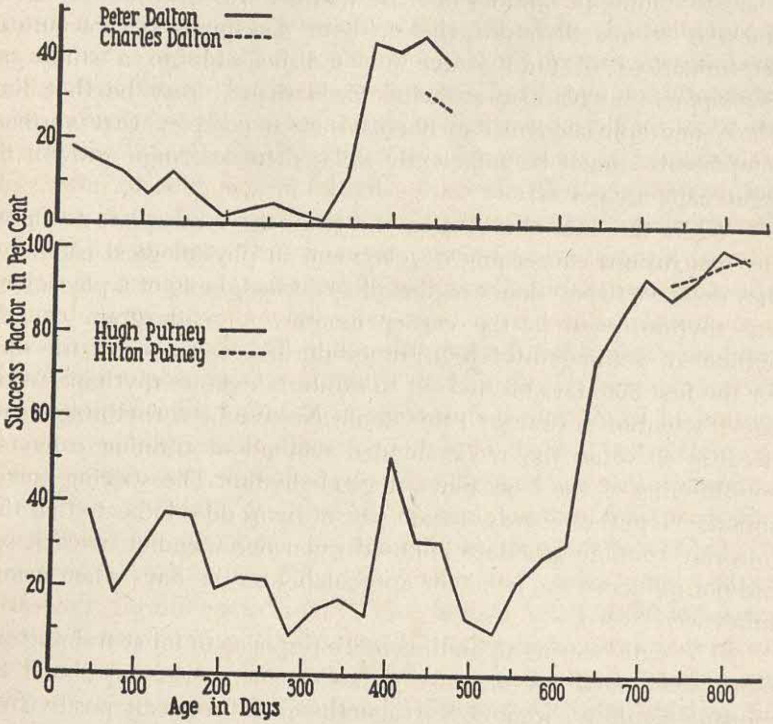

Figure 2. Achievement curves indicating the percentage of successful responses to the toilet situation as manifested by all four children.

The curve of Hugh Putney shows a second rapid rise shortly after he was five hundred and thirty days old; by the time he was two years old he was stabilized within the ninetieth percentile. At the age of two years, his twin, Hilton, was exposed to the training schedule. Even at the start, Hilton's achievement was slightly above that of his twin who, from the age of forty-one days, had been subjected to systematic training. The hourly training schedule was continued with both children for four months, a period long enough to indicate that Hilton, as well as his brother Hugh, was stabilized within the ninetieth percentile. There is no evidence that this result was influenced by happenings when the children were not in the clinic. Throughout the first two years the mother cooperated by withholding any training of Hilton when he was at home. In the clinic, however, although he was never allowed to use the chamber, he did have occasion to observe other children urinate in a vessel.

Separate studies by Tilney (8) and Conel (3) on the structural development of the human brain indicate that no part of the cortex is functioning appreciably at the time of birth. Cystometric studies by McLellan (6) offer additional evidence that the part of the cortex governing micturition is not functioning in the infant even as late as six months of age. This investigator observed uninhibited reflex activity of the detrusor urinae in six infants ranging in age from two to six months. In making the cystometric examinations it was noted that rhythmic contractions during the fill are common to infants of this age range. Similar contractions were observed in adult patients suffering a supranuclear lesion; moreover, these patients exhibited a type of micturition simulating that of the infant. In contrast, the type of micturition shown by patients with lesions in the lower centers in no way resembled the infantile reflex. Therefore, rhythmic contractions during the fill and an infantile type of micturition were considered to indicate the absence of inhibitory influence from the cerebral cortex. In the light of these considerations, it can be assumed with reasonable certitude that the cerebral cortex exercises no influence upon bladder functioning of the newborn infant.

In view of the evidence that cortical centers are not functioning in the young infant, the relatively high level of the achievement curves of both Peter Dalton and Hugh Putney during the early weeks warrants consideration. It might be contended that a certain amount of "conditioning" of the reflex mechanism takes place at an infracortical level. On the other hand, it is known empirically that any mild stimulation is likely to activate voiding in the young baby. It seems reasonable to assume that the necessary handling of the baby in preparation for the chamber provided the requisite stimulation for "catching" a few specimens and raising the success factor during the early weeks. This slight elevation of the achievement curve suggests merely that the reflex mechanism governing micturition in the newborn infant is hypersensitive. The decline in the curve which follows is interpreted as an indication that the reflex mechanism had become less sensitive to vicarious stimuli, but that specific association with the situation was not evident. It may be that the loss of sensitivity of the reflex mechanism represents an inhibitory influence of higher center upon the reflex activity per se, since, from other studies of neuromuscular development a diminution of reflex activity is one of the earliest indications of cortical development. This type of inhibition should not be confused with voluntary inhibition of micturition which develops much later.

The sharp rise in the curve between ten and twelve months corresponds to the onset of cortical participation in the function and the association of the act with a particular situation. It has been observed that the rise in the success curve occurs about the time the child begins to pay unmistakable attention to the act of voiding and to the result. It is at this time that he may be found playing in the puddles or in other ways indicating an association of the act with the specific situation; he may listen to the sound, or even a glint in the eye may reveal his awareness of the act. It is possible that this period corresponds to the maturation of cerebral sensory centers which denote bladder sensibility. There is probably some, although incomplete, development of cortical participation in movements involving other muscles in the region of the pelvis and lower extremities are evident. This parallel development further suggests that some cortical control of the sphincters may be gained at this time.

There is a definite decline in the achievement curve following this first manifestation of cortical participation. From observations of general behavior the decline appears to correspond to a period of rapid advancement in associational behavior and the inception of discriminative and generalizing powers. This decline in the achievement curve warrants special emphasis. It is not, in the opinion of the investigator, an incidental phenomenon, but represents a phase inherent to the process of development. During the period of transition from simple direct associations to more complex discrimination processes, the original associations are often interrupted. This temporary loss of a definite association as the ability to generalize develops has been observed in connection with other types of behavior.

The subsequent sharp rise, which occurred in the case of Hugh Putney around twenty months and was stabilized around twenty-four months, indicates the actual functioning of the maturing powers of discrimination and generalization; that is, the ability to generalize and discriminate becomes integrated with the act of micturition. The child begins to learn that it is not merely a particular vessel, but a kind of receptacle or situation, that is suitable for voiding. It is at this time that the child may point to the vessel or in other ways take some initiative in indicating his desire.

Under ordinary circumstances there is probably another regressive phase after the child has begun to articulate and take the responsibility for excretory needs. The conditions of this investigation (exposure to the toilet situation at hourly intervals) were such that the child was never called upon to exercise his own responsibility. With other children it has been observed that they accept the responsibility confidently for some months, but as their horizon of interest widens, there again comes a time when they verbalize the need immediately after the act rather than before. The regression at this later stage appears usually when the child shows a beginning tendency to engage in fantasy and other signs of mental activity concerned with things outside the realm of the immediate. At the inception of development in projective and retrospective mental processes of this order, the child is unable to bring all of the newly developed cortical centers into timely and well-integrated functioning. A regression of this type does not reflect retarded maturation of the urinary system or of those neural centers which operate in its control, but rather the lack of integration of the newly acquired cortical functions with the cortical centers controlling micturition. Subsequently, the ability to integrate multitudinous and remote interests with physiologic responsibilities is established. Emphasis is made of these regressive phases because socially and pedagogically they are the most troublesome and the least understood periods.

TOILET TRAINING

Current theories and practice concerning the best method of training the young child to achieve bladder control vary considerably. Some authorities recommend the induction of a training schedule during the early months. Following a study of groups of children, Scoe (7) says: "Training for bladder control may be begun as early as the third month. Since infants left untrained after the seventh or eighth month are conditioned with difficulty to the use of the toilet, it is important that training be started during the first six months of life, even if such training cannot be conducted in a highly systematic manner." Blatz (2), despite his caution against overtraining, suggests that, up to the age of six months, the child should be placed on the toilet on rising, after meals, bathing, morning and afternoon naps, and at bedtime in order to acquaint him with the essentials of a voluntary process. Some of the more recent publications warn against the introduction of a training schedule too early. Aldrich (1) stresses the importance of waiting "for the necessary degree of development" and suggests that the time is probably ripe when the child is able to balance firmly on his legs. Gesell and Ilg (5) have wisely proposed certain behavior symptoms as a basis for judicious training: "Early training should be undertaken with moderation, without zealousness and also without solicitude.

The mother may take note of the natural rhythms of micturition and of the times of the day when he responds most promptly to a routine toilet placement. The mother should especially watch for the time when he wakes dry from his nap. This is one of the truest indications of a maturing ability to withhold micturition. It may occur during the last quarter of the first year, but usually is not established before thirteen to fifteen months. When the capacity to withhold has been attained, the response to the toilet usually is immediate, and a previously resistant child becomes compliant to the situation. At this same period the infant shows more awareness of wetting and of the puddles that he produces on the floor. This is a new level of discrimination and gives him a new chance to learn. He has first to know that he has urinated before he can tell that he is going to urinate."

Although there exists difference of opinion concerning the optimum time for the introduction of a training program, there is general agreement among many writers that the training schedule should be based upon the child's own physiological rhythm. The assumption that the baby shows a rhythm of periodicity in voiding implies that it would be reasonable to expect micturition to occur at fairly regular intervals over a period of time. Such intervals would presumably show regularity with respect to the clock or the sleeping or the feeding schedule. The data obtained from the four children in this study fail to reveal any tangible evidence of regularity of this order which could be used as a guiding principle in formulating a training program.

Concerning the inception of a training program, the results of this investigation indicate that early toilet training is, to say the least, futile. The small percentage of successful responses to the chamber which may be obtained during the first few months is misleading, as it probably reflects hypersensitivity of a reflex mechanism and not true "conditioning" or association with the situation. It is preferable to postpone training until evidence of cortical participation in the act of micturition is manifested. This stage can be detected in the behavior of most children by the obvious attention they give to the act and to the result. The child probably suffers no handicap in the achievement of bladder control if training is delayed beyond the onset of cortical participation, that is, until cortical control of the act is fairly well established. This statement is substantiated by the record of Hilton Putney. One reason for recommending the introduction of a training program at the inception of cortical participation rather than delaying until cortical control is established is to relieve the toddler of diapers. Furthermore, it is desirable to particularize the act of micturition at this stage in order to encourage the exercise of the newly acquired sensory-motor experience. However, if training is initiated at this time, it should be done with the understanding that a period of decline or regression in the successful responses to the situation may follow before cortical control of the activity is stabilized. This period of regression may extend over several months. If it is recognized that periods of regression are a part of the process of growth and that they coincide with rapid advancement in other aspects of development, then it is possible to handle them more judiciously.

No mention has been made of nocturnal control over micturition because that phase of development was not included in this investigation.

SUMMARY

Two sets of identical twin boys were under intensive study during 17 and 28 months, respectively, for the purpose of determining basic behavior changes accompanying voluntary control of micturition. Such an analysis of behavior development offers a foundation for a more detailed correlation of structure and function when the underlying neural mechanisms have been established. A second purpose was to ascertain whether or not a systematic "training schedule" appreciably alters the course of development. To this end, one member of each pair of twins, after the first month of life, was subjected to a program of being placed on the chamber at hourly intervals during seven hours of the day. Their twins were not allowed on the chamber until they were fourteen and twenty-four months of age, respectively, at which time they were exposed to the same schedule of toilet training. In both cases the achievement of these boys approximated that of their brothers, even at the onset of their training schedule.

The achievement records of the two children who were given systematic "training" indicate that there are several distinct phases in the attainment of bladder control and that these phases are not altered by the imposition of a training program. During the early months of life the reflex is so hypersensitive that any mild handling or disturbance of position may initiate voiding. Subsequently, the reflex is less sensitive to vicarious stimuli. This period is represented in the achievement curve by a decline in the percentages of successful responses to the toilet situation. The onset of cortical participation is indicated by a sharp rise in the achievement curve and is reflected in the child's behavior by unmistakable signs of awareness of the act and the result. This rise in percentage of successful responses is a transient one; a regressive phase follows which apparently coincides with a period of rapid advancement in complex mental processes involving not only simple associations but discrimination and generalization. As these processes advance to a stage of functional integration with other activities, there occurs a second rapid rise in the achievement of bladder control. After this period the child's performances are stabilized at a fairly high level. However, there may be other minor regressive phases; an explanation of these has been proposed, although the conditions of this study precluded their manifestation.

A discussion of the optimum time to introduce toilet training has led to the suggestion that it be delayed until the child's general behavior indicates cortical participation in the act of micturition. Even then, periodic regressive phases are to be expected. Such regressions are temporary and apparently coincide with rapid advancement in other aspects of development.

Measurements of the frequency and the amount of voidings during one day of each week yielded no evidence of a basic physiological rhythm which could be used as a guiding schedule in setting up a training program.

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Three—An Experimental Study of the Nature of Improvement Resulting from Practice in a Mental Function

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The design of the following experiment is very similar to that of Section One in this chapter. An added feature is the retraining of both the experimental and control groups several months after the first phases of the study had been completed.

THE PLAN OF THE EXPERIMENT

The present experiment was designed to test, in some measure, the nature of improvement in a mental function. For this purpose, it seemed advisable to use as subjects persons in whom growth was by no means completed and in whom growth was going on, presumably, with great rapidity and a mental function which had been little practiced and in which, presumably, the acquisition of technique would be of rather small amount. After some preliminary study, we selected as subjects a group of children, ages four years to five years, eight months from the kindergarten of the Horace Mann School and as a function, memory for series of digits presented orally. Since this function constitutes one of the recurring tests in the Stanford-Binet scale, the results should throw some light upon the nature of the functions which are accepted as criteria of intelligence.

The experimental procedure called for two groups of subjects equivalent in abilities related to memory for digits, one of which was to be trained intensively and the other tested only at the beginning and end of the practice period. From a larger group of children, two groups were made up by matching each child in one group with a child in the other as nearly as possible the same in each of the following traits:

1. Sex
2. Age
3. Mental age on the Stanford-Binet test
4. Intelligence Quotient
5. Scholastic maturity as judged by teachers
6. Memory for digits, presented orally
7. Memory for letters, series presented orally
8. Memory for series of unrelated words, presented orally
9. Memory for series of related words, presented orally
10. Memory for a series of 10 geometrical figures, each presented visually for 5 seconds. After all were presented, a test of the recognition type—originals mixed with new designs—was given. Tests were given to pupils individually.
11. Memory for seven pictures—boy, hat, cap, and so on presented visually on a card strip for 15 seconds. Test by recognition method. All tests given individually.
12. Memory for picture "names." A series of 10 drawings of common objects with its common name under it in type. Task is to learn the "name" of each picture. Each card is presented 5 seconds: test by selecting words studied from a group including the old and new ones. Individual tests.

Two groups of 16 pupils, approximately matched in these traits and abilities, completed the experiment. The averages of these groups in the measurable traits are shown in Table I. It may be

TABLE I

SHOWING AVERAGE SCORES OF PRACTICE AND CONTROL GROUPS IN THE INITIAL TEST

Traits	*Age* [a] *Oct. 1*	*MA* [a] *Oct. 1*	*IQ*	*Memory Digits*	*Memory Letters*	*Memory Unrelated Words*	*Memory Related Words*	*Geometrical Figures*	*Pictures*	*Picture Names*
Practice group	5.1	6.31	122	4.33	3.64	3.86	14.0	4.3	5.3	7.5
Control group	5.1	6.35	123	4.33	3.71	4.07	13.7	4.0	5.7	7.0

[a] Age and mental age as of October 1, two months before the study was begun.

seen in the table that, in the averages, the two groups are substantially equivalent at the beginning of the experiment in the important traits taken into consideration.

THE RESULTS OF SPECIFIC PRACTICE IN MEMORY FOR DIGITS

Beginning shortly after the completion of the initial tests, the pupils in one group were given, individually, on each available school day, practice in immediate memory for digits. A large number of series of digits, arranged by chance, were prepared and presented according to the method prescribed in the Stanford-Binet scale. Each day, the pupil began with a series shorter than the largest one on which he had succeeded in two out of three trials on the preceding day. Each child, then, on each day was tested on three series of each length from a short series to a series on which he failed in two out of three attempts. The results are based on the number of digits in the largest series in which the subject was successful in two out of three attempts. In the group of 16 from which the averages were computed, all attended fairly regularly. In the case of absences, the pupil was given the score earned on the preceding test. The training was continued until May 20, the end of the school year, 78 days of practice.

The trained group progressed steadily from an initial score of 4.33 to a final average score of 6.40 digits—a gain of 2.07 digits. In the Stanford-Binet scale, 4 digits is placed at year four, and 6 at year ten. The practice group, then, advanced during a period of 4.5 months during which they practiced on 78 days, an amount equal to that which the average untrained child advances in approximately six years.

The control group was given the test on the first and last of the 78 days. The average score on the first test was 4.33 and on the last, 5.06, a gain of 0.73 digits. The gain of the practice group is appreciably greater than that of the untrained group.

THE INFLUENCE OF DISUSE ON IMPROVEMENTS

The problem, now, is to determine whether the improvement in the case of the trained group is due entirely to acquired techniques in handling digits and adjusting to the test conditions, or partly or wholly due either to the improvement of capacities underlying immediate memory for digits by some direct means or to capacities improved indirectly by a stimulation of growth, or to both. We therefore decided to apply, as one means of discovering the character of the improvement, a test of the influence of disuse.

One October 10, 1924, approximately four and a half months after the final practice day in May—a period approximately equal in length to the practice period—14 pairs of the original group of 16 pairs were found and tested. The results for the two groups were as follows:

TABLE II

	Practice Group	*Control Group*
Average score, initial tests, 12/20/23	4.36	4.41
Average score, final test, 5/20/24	6.36	5.08
Gain, during period of practice	2.00	0.67
Average score, test after disuse, 10/10/24	4.71	4.77
Gain over initial test	0.35	0.36

This result is most significant. While the practice group at the end of four and a half months of training excelled the control group by an appreciable amount—an amount equal to about six years of average growth according to the Stanford scale—after four and a half months of disuse, the advantage has been lost completely, the two groups were as nearly equal as they were at the beginning of the study.

As we see it, the experiment indicates that in the case of memory for oral digits among these children, at least, improvement brought about by four and a half months of practice, while appreciable, is due not to capacity increased directly or indirectly by means of accelerated growth but exclusively to the acquisition of technique. What constitutes technique, the experiment does not disclose. The factors may be better habits of attention under test condition, adaptation to the examiner's signals and voice, the elimination of strain or anxiety, or more subtle devices utilized in keeping the digits in mind, in seeing relations among them, in visualizing them or whatnot. Whatever they may be, the study indicates that they are rather unstable at least in the sense that all the skills and mnemonic devices acquired were lost during an equal period of disuse.

Another fact also suggests that the techniques were unstable. The tests in October, 1924, were given by a different—but skillful—experimenter from the one who gave the earlier tests. As shown in Table II, the average scores of both groups in the last test were smaller than those obtained four and a half months before although somewhat better than those of the initial tests ten months earlier. The suggestion is that improvement brought about by practice consists in part of adaptations to the voice, mannerisms, and features of the test technique of the examiner.

RESULTS FROM THE SERIES OF TRANSFER TESTS

As a further check upon the nature of improvement, the series of memory tests, given before practice in digits was begun, were repeated in October, 1924. The results of these tests are shown in Table III.

TABLE III

Showing the Initial (I) and Final (F) Scores and Gains (G) in Several Tests of Memory for the Practice and Control Groups

	Letters	*Unrelated Words*	*Related Words*	*Geometrical Figures*	*Pictures*	*Picture Names*
Practice I	3.64	3.86	14.0	4.3	5.3	7.5
Group F	4.11	3.86	16.2	4.9	6.8	9.0
G	0.47	0	2.2	0.6	1.5	1.5
Control I	3.71	4.07	13.7	4.0	5.7	7.0
Group F	4.28	3.93	17.2	5.2	6.7	8.8
G	0.57	0.14	3.5	1.2	1.0	1.8
Difference in favor of practice group	—0.10	0.14	—1.2	—0.6	0.5	0.3
PE difference	0.30	0.25	1.8	0.4	0.6	0.7

The differences in gain between the two groups are inappreciable; each group shows in about half of the tests, a slight but really unreliable superiority such as should occur if the groups were substantially equal. There is, in other words, no evidence that the prolonged training with digits has brought about any permanent improvement in immediate memory for other materials.

RESULTS FROM A SECOND PERIOD OF PRACTICE

After reviewing the results of the "retention tests," that is, those given after the interval of disuse, the fact that they were not extensive led us to decide to give a longer series, a series which really amounted to another practice or "relearning" period.

Eleven of the originally trained pupils, all that were available, were matched as nearly as possible in sex, age, mental age, intelligence quotient and memory for digits, with 12 others, 8 of whom were members of the original control squad. A comparison of the two groups is given in Table IV.

TABLE IV

AVERAGES FOR THE TWO GROUPS IN JANUARY, 1925

	Age	*MA*	*IQ*	*Digits*
Original practice group	6.34	8.1	128	4.73
Unpracticed group	6.46	8.2	126	4.83

Beginning January 27, 1925–13½ months after the beginning of the study, 8 months after the end of the practice period, and 3½ months after the "retention" tests—both squads of children were given 22 days of practice in memory for digits somewhat more intensive than before. Each child was given three series of digits from lengths well within his grasp to a length of which he missed two or three times in three trials. This procedure was repeated three times on each day, that is, nine series of each length. The scores used for securing the averages were the number of digits in the longest series on which the subject succeeded in two-thirds or more of the trials. These exercises were conducted by two experienced examiners—each taking half of each group—who had given none of the earlier tests to these pupils.

The results, in terms of the average daily scores for the 22 practice days are shown in Table V.

Table V shows essentially equal improvement for the two groups. There is no evidence that the 78 days of training which ended 8 months earlier had brought about any improvement in the fundamental capacity underlying memory for digits either in some direct way or, indirectly, by the stimulation of growth of these capacities.

TABLE V

AVERAGE DAILY SCORES IN MEMORY FOR DIGITS

Day	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	Day 22–Day 1
Practice group	4.73	4.82	5.00	4.82	4.73	4.82	4.91	5.09	4.82	5.27	5.27	5.18	5.36	5.27	5.45	5.18	5.36	5.54	5.54	5.73	5.63	5.73	1.00
Unpracticed group	4.83	4.83	4.93	4.93	4.93	5.00	4.93	5.17	5.00	5.25	5.33	5.33	5.25	5.42	5.50	5.58	5.66	5.66	5.75	5.83	5.92	5.92	1.09

SUMMARY

The main facts produced by the experiment are as follows:

1. Practice in immediate memory for digits on each of 78 days during a period of four and a half months by young children results in a marked gain in ability.

2. A group of children of equal ability in memory for digits and in sex, age, mental age, IQ, and in other forms of memory who were given no practice were, when tested at the end of the practice period, better than at the beginning but clearly inferior to the practice group.

3. After four and a half months of no practice, the two groups were again equal—the practice group had entirely lost its advantage. They were also equal in tests of immediate memory for other materials.

4. After three and a half more months without practice, both groups were given 22 days of intensive training at the end of which the two groups were still approximately equal. Neither in results (3) or (4) was evidence of any permanent effects of the 78 days of practice found.

The facts, we interpret as follows: The improvement brought about by specific practice is due to the acquisition of special and subtle techniques of work, to adjustments to the test conditions, familiarity with digits—to acquired information and specific methods of attack. These special techniques and mnemonic aids seem to be unstable and transitory inasmuch as after four and a half months of disuse they had disappeared.

Since the effects of the intensive training are so evanescent and since no permanent results of any kind favoring the practice group were found, the conclusion suggested is that training, under the conditions of the study, produced no increase in the capacities which underlie the function either in some direct way, or indirectly, by means of the stimulation of growth. The improvement brought about by practice seemed to be wholly in the form of devices, information, adjustments to the test conditions, "tricks of the task."

The demonstrated unstable and transitory character of improvement in memory for digits indicates the desirability of obtaining, in other studies of similar skills, a measure of the permanence of the improvement brought about by practice. Are the well-known increases in efficiency in memorizing poetry and prose, in solving verbal and mechanical problems, in rate of reading of several types which are usually rapidly produced by specific practice as evanescent as the improvement secured in this study? Or, is such instability peculiar to but few functions or is it a unique characteristic of the learning of very young children? The present material, so far as we know, gives no satisfactory answers to these questions.

Four—Later Development of Children Specially Trained During Infancy

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The three studies thus far presented in this chapter have been primarily negative with regard to the retention of permanent advantages from early training. However, it may be that some activities profit from early training, whereas others do not. Perhaps the three studies which we chose to present first represented activities in the later group.

McGraw trained a set of twins in a large number of diverse activities. A full account of her experiment is presented in GROWTH: A STUDY OF JOHNNY AND JIMMY. *New York: Appleton-Century Company, Inc., 1935.*

In brief, Johnny was given a long training period in the laboratory starting as early as possible and continuing until he was twenty-two months of age. Jimmy was given no laboratory training until he was twenty-two months of age when he was given two and a half months of intensive training in the same activities in which Johnny had been trained.

Originally the twins were diagnosed by competent authorities as identical twins, and the experiment was planned to compare the early training period of one twin with the later training of his identical brother. It became apparent, however, that the original diagnosis was in error and that the twins were nonidentical. Although this unfortunate error caused the study to lose some of the value that it otherwise would have had, it nevertheless yielded data of importance.

The following report deals with the results of early training *four years after the training was completed; that is, when Johnny and Jimmy were six years of age. The student will find that McGraw believes that consequences of certain kinds of early training were observable at six years, whereas the outcomes of other training were no longer evident.*

INTRODUCTION

In 1932 Johnny and Jimmy, twins, became the subjects of an intensive study of behavior development at the Normal Child Development Study of the Babies Hospital, Columbia-Presbyterian Medical Center. Before the study had been under way for two years, it attracted wide interest because of the reports of a baby less than a year old swimming with his face under water, ascending steep inclines and by the time he was sixteen months old moving around with considerable skill on roller skates. These were the achievements which gave the study a popular interest, but such performances were incidental to the primary objectives which were (a) to analyze the sequential phases or changes through which a growing infant passes in the achievement of a given performance, and (b) to determine whether these phases are altered by certain prescribed conditions, the daily stimulation of activity on the one hand and the restriction of activity on the other.

Such investigation was timely because of the current pediatric and psychiatric notion that infants should not be overstimulated and because of a general assumption following a co-twin study by Dr. Gesell (2) that the immature nervous system of the infant is unresponsive to practice effects and that development during infancy is essentially a matter of neural maturation. It is proverbial that among older children and adults practice leads to improvement in performance. If the infant is unresponsive to practice effects, and if, on the other hand, the adult is responsive, it is a reasonable assumption that there must come a time in the course of the child's development when improvement by virtue of experience begins. It therefore seemed to the writer that if one began soon after birth to stimulate one member of a set of twins in certain activities and within certain limits to restrict the activities of the other, it might be possible to ascertain that period in life when the individual begins to profit by experience or repetition of performance. Johnny and Jimmy were selected as the subjects for this investigation and a group of 57 infants, examined at weekly or biweekly intervals, served as controls. The general procedure during the first 22 months of life was to observe both babies in the laboratory five days a week from nine until five. During this time Johnny was stimulated daily to engage in activities to the extent of his capabilities, whereas Jimmy was left with a few toys unhindered in his crib except for disturbances accompanying routine care. When they were twenty-two months old, Jimmy was given a period of two and one-half months of intensive practice in those same activities in which Johnny had been given earlier and more prolonged exercise. A detailed report of this investigation was made in 1935 in *Growth: A Study of Johnny and Jimmy* (3).

In this report it was pointed out that there is no one age period or developmental stage which clearly demarcates an earlier state of immaturity during which the child is incapable of improving through practice from the subsequent state in which improvement through practice becomes feasible. The impossibility of identifying such a critical period in the development of the individual results, it would seem, from the fact that the nervous system does not mature uniformly. There are critical periods dependent upon the maturational status of the nervous system, but these periods vary with respect to the particular activity under consideration. Before training or practice can be economically provided, it is essential to determine, by the observation of behavior symptoms, the periods of greatest susceptibility for each type of activity. It will be recalled that while Johnny was induced to roller-skate with considerable skill by the time he was sixteen months old, repeated daily practice in a seemingly simple activity like tricyling evoked no improvement either in technique or performance until he approached nineteen months of age. The discrepancy in these two activities affords a striking example because under ordinary conditions a child learns to tricycle much younger than he does to roller-skate.

It has been four years now since this special study of Johnny and Jimmy was discontinued. Although they have at specified intervals during these four years been given follow-up examinations at the laboratory, their life at home has otherwise been comparable to that of other New York children of their socio-economic status. However, an immediate question arises as to the sequelae of their contrasting experiences during the first twenty-two months of life. Now that they are just attaining school age, when a new chapter of a child's life is opened, it is desirable that we consider their relative development to date in the light of their earlier experiences.

The early investigation brought out the fact that certain activities of infants can, through exercise, be brought to a much higher level of achievement than is normally assumed, but it will require new and longer studies to determine the extent to which practice should be enforced in order to render its results comparatively permanent. It is well known that many adults who learn a performance such as bicycling in childhood can, after a lapse of years without practice, pick it up in fairly short order. On the other hand, an adult resuming his attack on a language which he has not spoken since early childhood will show more than an initial awkwardness in relearning the language although he may acquire it with greater facility than someone who had never been exposed to it before. In other activities an adult often appears to be a virtual tyro despite his childhood accomplishments in a given field. An adult's loss of childhood skills in marble shooting, ball throwing, and the like, is such a common occurrence that it is a familiar subject for the cartoonist and other humorists. General conclusions concerning the permanent effects of practice are therefore impossible since different types of skills exhibit wide variation in their tendency to be retained or lost.

In evaluating the permanency of practice effects with Johnny and Jimmy, we shall consider first those laboratory activities in which Johnny had been given long and intensive training during infancy but in which Jimmy had received only two and one-half months of intensive training beginning when he was twenty-two months old. During the past four years, they have been examined in these same performances at intervals varying from two weeks to six months.

LABORATORY STUDIES

Tricycling. One of the most interesting attainments of Johnny and Jimmy during their period of intensive study was that of manipulating a simple tricycle. The initial practice period was begun with Johnny when he was eleven months old. It was pointed out in *Growth* (3) that this activity was initiated before his neuromuscular mechanisms were ready for such a performance, as it was eight months before he began to show distinct comprehension of the situation. After he showed some degree of mastery he acquired an easy, skilful performance in about two months. It was also pointed out that Johnny had apparently suffered by his long and futile practice periods. Jimmy, whose training began when he was twenty-two months old—presumably when his neuromuscular mechanisms were in a state of readiness—accomplished an easy performance in a shorter time than did Johnny. After the period of intensive training was discontinued, both boys were observed at the laboratory in their tricycling behavior, and at no time did either of them show any distinct loss in this particular skill. The twins were living at home during this time. Although they did not possess a tricycle in their own home it was impossible to ascertain whether they had had access to one in the nursery school or on the playground. On the other hand, it may be that tricyling, like bicycling, automobile driving, and so on, is a type of skill which does not deteriorate appreciably through lack of practice once it has been definitely acquired.

Skating. In some of the other gross motor skills we find a different story. It was pointed out in the earlier report that Johnny, who began his roller-skating experience when slightly less than a year old, enjoyed an advantage over his twin, whose practice in this activity was begun at the age of twenty-two months. This advantage appeared to be attributable to several factors. In the first place, from purely mechanical considerations, a child who is just learning to walk possesses because of his relatively wide base and short legs greater static equilibrum than does the child who is older and therefore taller (4). In the second place a child who is just beginning to walk, has ample experience in falling, less distance to fall, and is therefore less disturbed when he falls on skates than is the older child. Furthermore, a child's achievement in a particular skill like skating is determined to some extent by the number of distracting or interfering interests which play upon a performance at a given time. The toddler of a year is less responsive to the world about him than is the child of two years or older. Johnny was at the threshold of independent walking when the skating practice was initiated and apparently experienced an advantage in static equilibrium. Also, having less general experience at the age of twelve months, he was less hindered by distractions and emotional factors and therefore could exert himself to the limit of his neuromuscular abilities, whereas Jimmy's performances at the age of two years were modified considerably by interference from broader interests, so that his activities at the time were not the result of optimum neuromuscular coordinations. Even after a practice period of two and one-half months, Jimmy had still not acquired a well-integrated skating movement.

The children were given skates as a present shortly after their daily laboratory visits were terminated, but these skates were demolished within a few weeks and their parents report that they have had no other skating experience except for the follow-up laboratory tests. Their performances on these occasions corrobate the parents' report. In brief summary of the notes which have accrued on their skating behavior during these four years, it may be said that soon after their practice period both children began to show a loss in skill and disorganization of the skating pattern. When the children were about three years old, it was noted that the chief source of their difficulty was in maintaining the erect posture when on skates. The aspect of balance had undergone greater disorganization than the actual progressive movements. Disorganization of the skating behavior was more pronounced in Johnny's performances than in Jimmy's. This difference, however, appeared to be due to Johnny's attitude of abandon and a tendency to lunge forward even though he had lost in equilibrium, whereas Jimmy adhered more to the short, stiff strokes characteristic of his early practice period. Despite this difference both children began to show an increased tendency to lose their balance and tumble, when compared with their earlier performances. Neither of them skates well today. The fact that the children showed no loss of skill in tricycling but complete disorganization of roller-skating raises the question as to why certain skills deterioriate through lack of exercise and others do not. The answer to this question is, of course, problematical since the factors which control a growing behavior are multiple and were not in this study controlled by laboratory measurement. However, from observations of other children in our study as well these particular boys it seems clear that at least three factors play an important role in determining the permanency or deterioriation of a motor skill when no special exercise of the function has occurred during a period of years. The first factor is the lack of practice *per se;* the second is the influence of the child's attitude toward the performance, and the third is the changing configuration of the bodily structures as a result of physical growth. It is impossible to evaluate the effect of disuse of function without taking into account the factors of attitude and organic structures. From direct observation of the twins' skating behavior the writer is of the opinion that changes in bodily proportions were of considerable significance in the disorganization of skating behavior. Actually the child of five or six years, because of his relatively long legs and narrow base, has a set of structures with which to perform the task of skating different from those of the toddler. With the set of structures characteristic of the toddler, Johnny developed a well-coordinated skating movement. Johnny's attitude toward skating continued cooperative and favorable during the entire four years. There are other motor activities in which he showed no appreciable loss of skill despite the lack of exercise. It seems reasonable to infer, and certainly the character of his behavior indicated, that the disorganization was brought about not solely because of disuse of function but also because during the nonpractice period important growth changes occurred in the bodily mechanisms which function in the act of skating. It is reasonable to assume that if the practice had continued he would have altered his behavior gradually to meet the new and slowly developing structural demands. Jimmy, who was less skilful in skating than Johnny at the end of their practice periods, has also shown deterioriation. The fact that both began to show marked difficulty in balancing at about the same age lends support to the contention that physiological growth was an important influence in the disorganization of their skating behavior.

Slides. Another activity which seems to have undergone alteration in the course of years, though it was not so completely disorganized as roller-skating, was that of ascending steep slopes. It may be recalled that (3) at the end of the experimental period, Johnny was able to ascend easily a slope of 70 degrees and Jimmy could with somewhat less ease, scale the incline of 61 degrees. For some months immediately following their practice period Johnny showed, on follow-up examinations, no loss in motor skill in mounting even the steepest slopes. Jimmy, on the other hand, showed some initial loss, but then began to improve both in motor skill and in persistence. Alterations in their methods of ascending the slides became most obvious when the children were about three years old. At this time it was noted that they had great difficulty in managing their longer legs. They would attempt to ascend on their knees while grasping the ridges and pulling vigorously with their upper extremities. The most outstanding individual difference was Jimmy's persistence in trying to ascend in this manner while Johnny, after a few trials, would shift to the more efficient method of using his toes. He also climbs the steep slopes with greater ease and muscular coordination. He gives the impression of having better gripping power in his toes than Jimmy or most six-year-old children whom we have had occasion to observe in this activity.

At the present time the writer is less impressed as to the extent to which daily practice might have functioned in Johnny's achievements on the slopes than she was at the time of the original report. Practice undoubtedly operated in his ascending slopes of 61 and 70 degrees respectively. Other infants in our laboratory, however, have ascended slopes as steep as 40 or 48 degrees without great difficulty even when they had not been given systematic practice in such performances. To the experienced observer, it is obvious that infants can more easily ascend slopes of this order than can older children. The advantages enjoyed by the infant appear to be due somewhat to differences in body configurations. The center of gravity in the infant's body is relatively higher and his legs are relatively shorter. It is therefore possible for him to get his chest, and thereby his center of gravity, nearer to the slide without raising his pelvic girdle too high. It is also possible that the texture of infant skin is such as to create a higher friction coefficient between his hands and feet and the underlying surface. Whatever the reasons may be, the fact remains that the babies can stick to and ascend these slopes with greater facility than the older children. The difficulties of older children are manifested by slipping or a deficient gripping power in the toes and in managing their long legs. Alterations in the slide climbing behavior of Johnny and Jimmy during the past four years were of a similar nature. It is a reasonable inference, therefore, that growth changes in body proportions played a large role in altering their method of ascending slopes. Any permanent effects of the early practice which Johnny enjoyed are general, as indicated by superior motor coordinations, except for the comparative readiness with which he shifted from an inferior to a more effective method of managing his long legs and using his toes.

In descending these slopes neither Johnny nor Jimmy at any time showed any distinct loss of motor skill or alteration of method. During the first few months following the practice period Jimmy was more timid and required greater urging to descend but after the first three or four months he overcame this hesitancy and manifested no lack of motor skill subsequently.

Getting Off Pedestals. Postpractice behavior of the children in getting off pedestals is similar to that of descending the slides, since no definite deterioriation of motor skill was manifested by either of the boys. The pedestals ranged in height from 14½ to 63¼ inches. At no time during the four-year interim has Johnny shown any loss of skill or hesitancy in getting off these pedestals. At the time the practice period was terminated Jimmy was easily and deliberately climbing off the 63¼ inch pedestal. During the months immediately following his two and one-half month practice period, there appeared a regression in his general attitude or emotional adjustment to the situation which interfered with his motor performance. He would complain even on the lower pedestals and would refuse to get off the higher ones. Behavior of this character continued more or less until he was about four and one-half years old when his attitude seemed to change to cooperation and some enjoyment of the situation. The failure of Jimmy to get off the higher pedestals was not so much a deterioration of the motor coordinations formed during his practice period, but rather a recurrence of attitudes established prior to his practice period. Once he could be induced to climb down he could do so with considerable ease. He has never, however, attained the agility which Johnny manifests in this performance. At the present time the individual differences are indicated not so much in terms of their achievements as in the degree of coordination which they show when performing the same act. This superior motor coordination on Johnny's part is reasonably attributable, in a measure, to the difference in amount of exercise the children received in the activity during their first two years of life. Climbing off pedestals is another type of performance, the motor aspects of which do not suffer appreciable loss through lack of exercise once the motor habits are well established.

Jumping. In the report of early behavior of the children in the jumping activity (3), it will be noted that in this type of performance the attitude of the child played a major role in determining the somatic response. At the end of the experimental periods the investigator felt not only that Johnny had established a cooperative attitude, but that the integration of the essential movements in jumping had been accelerated. Jimmy, on the other hand, even after two and one-half months of daily exercise could not be induced to jump off a tall pedestal into the outstretched arms of an adult. It seemed unquestionable that Johnny experienced an advantage in this performance by having had practice in the activity before his increased perceptive and emotional capacities added complexities to the somatic or motor aspect of the behavior. During Jimmy's two and one-half months of practice (at two years of age) his attitude became definitely more acquiescent, but the alteration of attitude was not sufficient to effect an integrated jumping performance. At the end of the exercise periods, when they were twenty-six months old, Johnny was gleefully leaping from tall pedestals with even a slight "spring" as he threw himself forward. Jimmy, happy enough, would stand on the pedestal, shifting his weight from one foot to the other, squatting, and in other ways indicating his urge to go forward, but was not quite able to consummate the performance. During the first few months after their practice periods were terminated, Johnny began to show a less cooperative attitude and for a while there was even less motor skill in his action, that is, when he did jump there was noticeably less grace in his movements. This loss or deterioration was, however, temporary and intermittent, appearing at intervals over a period of six or eight months. After that relatively brief period jumping was again one of his most enjoyable activities and he subsequently showed no distinct loss in motor coordinations. It appeared to the experimenter that this early change in attitude was due in part to imitation of his brother's behavior rather than to real hesitancy in carrying out the act.

For some months after their practice periods terminated Jimmy showed a definite regression in his behavior. He would cling tenaciously to the adult, refuse to stand up on the pedestal and cry lustily. When he was about three years old, however, his attitude became more acquiescent, and his behavior comparable to that manifested during his exercise period. He would stoop or squat on the pedestal, reach toward the adult, and if the adult was only about eight or ten inches away he would throw his shoulders forward. He has steadily shown improvement and is now willing to jump, but he has not manifested the abandonment nor been willing to jump as far as Johnny. In this activity there has been a definite residual of the early training period in Johnny's favor.

Purposive Manipulation of Graded Stools. Growth (3) offers an analysis of the children's development in manipulating stools of various heights in order to obtain lures which had been placed out of reach. At the time the experimental or practice period terminated, neither of the children had attained the maximum degree of proficiency in this activity. At that time Johnny would juggle eight different pedestals, ranging from 7½ to 63¼ inches in height, in order to climb up and obtain an object hung some nine feet above the floor. However, he still had a tendency to push all the stools in a cluster, not seriatim, usually with the tallest one beneath the lure. On a few occasions he had shown some tendency to eliminate unnecessary pedestals, but this aspect had not become a fixed part of his behavior activity when the practice period ended. Jimmy, on the other hand, was, at the same time, able to manipulate two or three stools successfully in order to obtain the lure placed upon the 63¼ inch pedestal, but he had at no time successfully obtained the object hung from the ceiling, which arrangement demanded his making use of the taller pedestals for climbing. Johnny's practice period began in this activity when he was eighteen months old, and Jimmy's when he was twenty-two and a half months old. It was pointed out in the original report that the time span between the inception of the practice periods for the two children was not great and therefore came nearer to striking the critical period for both children, that is, that period when development in this type of activity would be most susceptible to advancement.

It would require too much detail in order to delineate the various changes in their performances in this situation during the four years they have been returning to the laboratory for follow-up examinations. It is sufficient to state that at the present time, as a rule, Johnny and Jimmy both arrange the pedestals purposively to obtain the lure when it is hung at least nine feet above the floor. Both children carefully discriminate in placing the tallest pedestal under the object. Johnny, however, carelessly and dexterously pushes all the other seven pedestals in a cluster about the tallest one with no definite order or arrangement. He then goes clambering up two or three of the stools, as may be necessary, for him to gain the top of the tallest one. He shows no hesitancy or difficulty in bridging wide gaps in order to pass from one stool to another when they are not in juxtaposition, and he shows no hesitancy in standing erect on the tallest stool in order to reach the lure. It never seems to trouble him that he has pushed all the pedestals in a cluster although he makes use of only two or three for the purpose of climbing.

Jimmy, in contrast, works diligently arranging the stools in a graded sequence with the tallest one carefully placed under the object. Once he has them arranged in a stairway he will climb up and stand on the one next to the tallest pedestal, but it is only rarely that he can be induced to climb up on the tallest one so as to obtain the lure. He has definitely less courage and less motor coordination in handling his body. It is the writer's impression that Johnny's failure to arrange the pedestals in order according to their relative height is a residual of his habits established earlier when his discrimination was inadequate to allow him to do so. The fact that it is not lack of discrimination at the present time was brought out one day when he was shown some movies of his performances taken before he was two years old. This particular reel included an occasion when he had arranged most of the pedestals in stair formation. Johnny's remark was, "That isn't me. I don't put them straight like that." With his greater agility he doesn't feel the need of an orderly arrangement, whereas Jimmy, who is more cautious in climbing, tries to make that aspect of the situation as easy as possible. The impression that the tendency of Johnny and Jimmy to employ all eight stools, even though three properly selected ones would have sufficed, is a residual of their early practice in this situation was substantiated by the behavior of a small group of children from six to eight years of age who had never been exposed to this particular situation before. At this stage of maturity these children would make use of only a few of the pedestals, even if they were not properly chosen. In other words, they had developed beyond the stage of thinking an additional one would help solve the problem.

Purposive Manipulation of Graded Boxes. This situation (3) called for the stacking of two or more boxes of different size on top of each other in order to obtain a lure suspended from the ceiling. When the practice periods were discontinued Johnny was skilful in stacking three boxes in order and had occasionally successfully stacked four boxes, but he was actually lacking in physical height to stack the fourth box easily. Jimmy, by contrast, at the same chronological age and after a period of two and one-half months' practice, had not arrived at the stage of placing one box on top of another for the purpose of climbing up to reach the lure. Therefore, in this situation neither child had attained his maximum development in performances of this order, though Johnny was considerably advanced beyond Jimmy.

In the four-year interim during which they have been given follow-up examinations in this situation Johnny has shown many fluctuations, the details of which would be too laborious for the reader if reported here. Jimmy was three years old before he began placing one box on top of another, and it may be that this is about the age when children would normally begin to engage in activities of this order if they were not given specific stimulation to do so. At the present time both children usually pile all four boxes on top of one another in order to obtain a suspended lure. Jimmy is more careful in arranging the boxes in order according to size, that is, with the largest one on the bottom and the smallest one on the top. He often shows a reluctance to climb up after he has neatly arranged the boxes. Johnny, who is careless and less discriminative in the order of arrangement, shows greater skill in climbing and maintaining his balance on even a quite unstable structure. It would seem that in this particular situation Johnny's advanced motor skill and courage operate to make him either careless or less discriminative of the relative sizes and placement of the boxes. Again, Jimmy, who has less courage in motor performances, is more meticulous in making arrangements, so that the motor aspect will be as easily accomplished as possible.

When a long-range view is taken covering the twins' behavior in these several laboratory situations during the past four years, it is clear that there are at least three different situations in which Johnny showed no loss of proficiency and in which any loss which Jimmy might have shown could be attributed more to his emotional or attitudinal status than to motor inadequacy. These activities are (a) tricycling, (b) getting off pedestals, and (c) descending slides. When we examine these activities we are struck with the fact that their mode of performance has suffered no major alteration. In other words, so far as the motor aspect of these performances goes, the twins had achieved a high degree of integration or maturity at the time the practice periods were terminated. Given relatively the same heights and slopes and a tricycle of relatively the same size, the two-year-old and the six-year-old child tend to get off pedestals or go down slides in essentially the same manner, and also to propel a tricycle in the same fashion.

In at least two different activities, and in certain aspects of a third, the mode of performance seems to have changed primarily because of alterations in the boys themselves as an aspect of physical growth, specifically because of their relatively longer legs and the shift in their centers of gravity. These alterations are especially noted in skating and in ascending steep slopes. The influence of changes in bodily growth upon behavior was also observed in the way in which the boys manipulated graded pedestals to obtain suspended lures. Actually, because of his short stature, the two-year-old child who pushes the tall pedestals nearer their base is less liable to tip them over and can, therefore, move them with greater ease than can the six-year-old whose contact point in pushing strikes the pedestal nearer its center of gravity.

Purposive arrangement of pedestals and manipulation of boxes were activities which were probably in the most fluid state at the time the special practice periods were terminated. For that reason the performances of both children have shown greater fluctuations in these situations than in activities which were more stabilized. While the achievement difference of the two boys was great when special practice was abandoned, their relative efficiencies have, during the past three years, approached each other though the two children adopt somewhat different methods in demonstrating their merits. As might be expected there is comparatively less diversity in their present achievements in those activities which were not so stablized when practice was abandoned.

Jumping from a tall pedestal seems to stand in a class by itself. It appeared that through practice the motor aspects of this performance had been greatly accelerated in Johnny. However, his performance had reached a high degree of fixity and suffered only a temporary loss immediately following the practice period, whereas the attitude which prevailed with Jimmy during his period of isolation had become so fixed that the two and one-half months of practice he experienced when he was about two years old were inadequate to counteract it. With increasing maturity Jimmy's emotional disturbance was somewhat abated, but it has never become sufficient to eliminate obvious muscular tension in his performance even though he will complete the jump.

Swimming. No follow-up study has been made of the swimming behavior. It will be recalled that the original investigation of this activity (3) terminated when the children were seventeen months old. During the ensuing four years there have been three or four occasions when their aquatic behavior was observed. Until the age of six years Johnny showed noticeably greater fortitude in jumping into the water and playing about, though he was not able and was not urged to engage in independent swimming. However, on the last occasion they were taken to a lake, when they were a little more than six years old, Jimmy was quite brave about jumping into the water with a ring around him for support, whereas Johnny showed some inhibition about doing so. Just why this change in attitude should have taken place is a matter for conjecture. An interesting difference was noted in their swimming strokes. When supported by a ring, both would venture out beyond their depth, but Jimmy maintained a vertical position as he made rapid treading movements with his legs. Johnny, on the other hand, assumed the horizontal position and engaged in graceful crawl strokes of the more advanced swimmer. Just why he should have taken up a method of movement which by experts is considered to be more efficient than the natural dog paddle of beginners is beyond present explanation as no attempt was ever made to teach Johnny strokes during his period of infantile swimming and at that time he used the typical dog paddle. It is unfortunate that this behavior could not have been studied further, especially since it appears that this is another activity in which the body proportions constitute a large influence in determining the manner of behavior. An infant whose head is relatively heavier with respect to his total body cannot maintain his face above the water level. Beginning swimmers between the ages of four and six years have a tendency to maintain a vertical position, if allowed artificial support, and their first movements without support are of the struggling order.

INTERPRETATION

In the light of their subsequent behavior, both in and out of the laboratory, it is safe to state that at the present time Johnny usually manifests greater motor coordination and daring in physical performances. Jimmy, who is more awkward and timid, exercises devious methods of rendering more easy the motor aspect of a given activity, so that both children may end up with the same result in terms of final accomplishment.

It has been pointed out that the degree to which an accelerated activity may retain its advanced status after the modifying factor of intensive exercise has been withdrawn is proportional to the degree of fixity the behavior pattern had attained when practice was discontinued. Fixity in this connection means whether the manner of performance had stabilized into a well-integrated movement. Examples of well-establized activities which manifested no major alteration through lack of practice are tricycling, getting off pedestals and descending slopes. Manipulating pedestals and boxes was in a fluid state, and their subsequent performances have shown many fluctuations.

Fixity in mode of behavior, however, is not the only factor determining the permanency of acceleration. Another important factor, organic change within the individual, has been recognized only by the analysis of later performances in skating and ascending slides. Johnny's mode of performance in these activities when he was two years old was highly skilful and of an order comparable to that of the efficient adult engaging in a similar activity. If one considered the activity in the abstract, one would say that his skating and slope-climbing behavior had attained a high degree of fixity. Yet these activities suffered alteration, or loss in skill. This loss of skill appears to have been due primarily to change in bodily proportions. Thus it seems that a child may, in a given activity, attain a high degree of skill and his manner of operation may be the most approved or efficient with the organic structure he has to work with at the time. If, however, at a later age he has a different set of organic structures for performing the same activity, then his behavior may show disorganization characteristic of the novice. Retention of general muscular coordination may persist but the actual patterning of the particular skill or behavior will be interrupted until the child has learned to operate the new set of body mechanisms. The comparative behavior of Johnny and Jimmy in roller skating illustrates this point. Even though the activity became disorganized in both children as they grew taller, superior general motor coordinations could be detected in Johnny's movements.

Johnny's superior motor coordination is evinced not only in adaptive skills as mentioned above but also in the common, more organic motor movements, such as the assumption of an erect posture, walking, and falling. Quantitative determinations of their efficiency in walking were ascertained periodically at six-month intervals from the time they were about two years old. While these measurements are too scattered to justify elaborate treatment, they do show that Johnny was consistently a little ahead of Jimmy, and to that extent these objective measurements corroborate the observational data.

In summary it might be said that the alteration or deterioration of performance, particularly motor performance, through lack of exercise or practice over a long period of time is determined (a) by the state of maturity the activity had attained at the time the practice was withdrawn, (b) by alteration in body mechanics or other physical changes which necessitate an alteration in the form of the behavior pattern, (c) by change in emotional or attitudinal adjustments which operate as an inhibiting or facilitating factor in a particular performance.

ATTITUDES

In evaluating the influence of attitudes upon somatic performances we have ordinarily considered only the attitude prevailing at a moment and concomitant with a particular motor behavior. It is well to consider also the persistence of a characteristic attitude which seems to predominate from day to day when the child is confronted with the same situation. That is, if a child week after week is placed on a pedestal and requested to jump to an adult and if on each occasion he stoops, clings to the adult and cries, then it may be said his characteristic attitude for the period of time is one which inhibits the motor performance of jumping. If, however, after a period of months he begins to show more fortitude not only in the jumping situation but in all comparable activities, then the possibility of general increase in emotional control warrants consideration. More specifically is the older child, because of his physiological maturity, capable of attitudes and adjustment which could not be evoked from the infant or younger child. It was pointed out in *Growth* (3) that an attitude, at a given moment, may arise from some developmental imbalance of the neuromuscular system. For example, the baby who pulls himself up beside the bed and stands crying lustily may do so because he has not developed the concomitant coordination of letting himself down to the sitting position. The attitude arises from the organic status of his neuromuscular development and not from some personal disagreeable experience associated with that situation. Likewise, it would seem, a change in attitude may be brought about not through direct and individual experience in a specific situation but through the integration of other aspects of development, that is, as a result of general physiological development. It is common observation that the period of greatest irritability, negativism, temper tantrums, and so on, is around twenty-four to thirty months. These common observations are substantiated somewhat by the investigation of Reynolds (6), who shows that there is a diminishing manifestation of negativism after two years of age. Anderson (1), interpreting Olson's (5), study of *Problem Tendencies in Children*, regards the period between two and four years as one in which "a fanning out of emotional life" takes place. It seems evident from Anderson's discussion that he has in mind not only diminishing emotional flare-ups in the older child but increasing differentiation and specificity of emotional expression. The older child gets angry at things which should make him angry, while it is common observation that the younger child may go into a tantrum when apparently everything is being done to appease him. It is also common knowledge that the intellectual and motor abilities of the two-year-old child have not attained equilibrium.

It seems reasonable to ascribe this characteristic and trigger irritability of the toddler to the many developmental behavior potentials which he has acquired during the first two years of life, but which have not been organized into adequate social responses. When these behavior potentials become integrated, then the child achieves a degree of emotional maturity regardless of his personal experiences in particular situations. In other words, emotional maturity may arise from increasing integration of intellectual and motor development as well as from direct personal and conditioning experiences. These statements sound tautological. It is difficult to make them otherwise since the different aspects of a behavior activity are interdependent and mutually influential. Illustrations may add some clarity to the analysis: A child who is just beginning to walk may leap off a tall pedestal without hesitancy because he has not developed adequate powers of height discrimination to appreciate all the possibilities of the situation. A little later, when his powers of discrimination have advanced, he squats and refuses to jump. Again it must be emphasized that his behavior has changed not because of an unfortunate personal experience but because of development in another aspect of his behavior. At this time he has adequate motor mechanisms for jumping but he has achieved powers of discrimination and for a time the one functions to inhibit the other. Subsequently when the two types of development become organized and well integrated he will again leap from the pedestal and his attitude is acquiescent or cooperative not for lack of discrimination but because he can exercise his motor functions in terms of his discrimination.

A study of the notes on Jimmy's attitudinal adjustment to the laboratory situations during the last four years indicates that a general maturing began to take place when he was about three years old. During the first year after he left the laboratory his attitude on return visits was infantile, comparable to that manifested during his period of restriction. He would cling to the experimenter, cry and refuse to go through with many of the performances for which he obviously had adequate neuromuscular development. At about the age of three years, he began to show a more interested and cooperative attitude; the change was shown in practically all the laboratory situations. His motor performances began to improve and have continued to improve to the present time.

Since no definite effort was made during this time to reeducate his attitudinal adjustments, it is assumed that the change was a result of gradual development in understanding and integration of his behavior in general. Jimmy's responses both in and without the laboratory are more representative of the typical child of his age than are Johnny's. This period of development in emotional and social adjustment achieved by Jimmy parallels the period for such development in children in general as ordinarily observed.

One attitudinal difference in the two children which was marked during their experimental period persists even today, though the difference is less marked than it was when they were about two years of age. Jimmy has always been more concerned with end result and Johnny with *modus operandi.* It cannot be said that Johnny's greater interest in the way things work rather than in obtaining lures arose from the more extensive laboratory experience. It is however reasonable, from the very nature of the laboratory experience, to assume that it was not without influence in developing such an attitude. Here he learned to work for the sake of performance even when the lure had no particular appeal. The fact that this difference in their interests was more marked during the experimental period than it is today would in part justify the opinion that the laboratory experience influenced Johnny's development in this respect. At the present time a sort of rivalry exists between them, but it is counterbalanced by their spirit of comradeship. In the main, they appear to be less sensitive to failure than most children of their chronological age who have come under the observation of the writer.

PERSONALITY DEVELOPMENT

In view of the importance attached to the early experiences of life in determining later personality makeup, and in consideration of the rather popular notion that overstimulation during infancy forecasts a neurotic child, two attempts were made to evaluate the personality and social adjustments of the children as of six years of age. Dr. William S. Langford, through psychiatric interviews with the mother and children, arrived at the following evaluation of the children's emotional and social adjustments as of today.

> It is difficult to discuss these two children without stressing the differences between them and without speculating as to possible causes of these differences. Johnny and Jimmy both are regarded by the mother as happy children who benefited by their experiences in the Normal Child Development Clinic. There are no particular difficulties in home management with either of the boys. The mother believes that both, as a result

of the clinic experience, are able to meet with people and adjust better in social contacts outside of the home than do her other children. When playing with the older siblings, they both are able to adapt their play to the situation; when playing with the girls it is dolls, and when playing with the boys it is guns and cowboys; they are equally happy at each type of play.

There has been a difference in the attitude toward the two children at home, in part coming out of the fact that Johnny was the "subject" in the clinic. This difference in attitude would seem important in the genesis of some of the differences between the two boys. Although the family tried to realize that the selections of Johnny for the conditioning experience in the clinic was not because of any favoritism, it was difficult for them not to favor Jimmy and not to try to make up for some of the things he did not get. During the latter part of the second year, after newspaper reports of the experimental studies had been published, they would, among other things, take Jimmy and encourage him to jump off the icebox. However, the experiences in the home, which would seem to have given Jimmy the greater security there, were more a result of circumstance. In their infancy when, at the end of the clinic day, they were returned home, Johnny would be tired and go to sleep; Jimmy on the other hand would be wide awake and ready for play and socialization with his parents and siblings. For a time Jimmy would strike Johnny and take away his toys; Johnny would not seem to dare to hit back. One wonders strongly if this, too, was not a result of attitudes in the home.

Jimmy seems to be more at ease in the home situation where he is the leader of the two boys. He usually bosses Johnny about. Jimmy is quite apt to come home with tales about Johnny and tell what he had done outside. The parents, one feels, do not particularly encourage him in this activity. Jimmy wakes up quickly and is on the go all day as a rule chattering a blue streak about whatever comes into his mind. He seems closer to his mother; talks more with her and likes to sleep with her. When he is put to bed with another sibling, he will frequently come into his mother's bed. He definitely prefers his mother to his father and feels that she likes him best. He is helpful about the house and likes to assist his mother in tidying up and washing dishes. Jimmy cries easily when things do not go his own way or if he is scolded. He indulges in mild temper tantrums consisting of stamping his feet when he cannot get what he wants. Earlier, at about two years of age, he went through a period of severe temper outbursts with breathholding.

Johnny gives evidence of some tension; he has always been a nailbiter, and the mother states that he is a "wiggler," cannot sit still in a chair and from time to time displays quite restless sleep. In addition, Johnny has always been enuretic nightly. This is difficult to interpret as enuresis seems to be a family failing. There was difficulty in establishment of the day habit with all of the older siblings and one brother did not stop his bedwetting until nearly eleven years of age. Jimmy still wets his bed occasionally although the mother did not tell of this until some time after the initial interview. In addition, an adult member of the family will occasionally wet his bed. The mother feels that this tendency comes from the father's family where all the members have a tendency toward urinary frequency and urgency. Johnny, among all of the Woods children, is a thumb-sucker; this began in early infancy and continues at bedtime even today although it used to be more marked and occur in the daytime also. He sucks the left thumb, and as an accessory movement pulls his own or bed partner's hair with the other hand; usually the latter. The sucking is rather vigorous and the accessory movement is so pronounced that most of the siblings prefer not to sleep with him. Johnny is somewhat ashamed of this habit and was disinclined to discuss it at first. Johnny is the quieter of the two children and rarely holds conversations with the other members of the family. Mother believes he is "deeper" and "when you least expect it Johnny will say something." He is dependent on his mother in being washed and dressed and tends to play in his bath. One feels that this slowness is more a result of preoccupation than of a desire to have the mother do these things for him as the mother becomes irritated at his slowness and finally does it herself rather than wait for him to complete it. Johnny is not thoughtful or helpful about the house, but tends to be "destructive and throw things around." The mother feels that Johnny has no fears and thinks that he would be better off if he were a little more cautious, especially in his attitude toward dangerous occupations, such as crossing streets. In the home, Johnny has a philosophical attitude and takes things as they come; he is not upset when he cannot get his own way. There are no temper outbursts and he rarely cries. During the past year he has shown a tendency to play with fire, but otherwise has shown no overt behavior difficulties. Both children when observed in the interview were friendly and cooperative and talked freely. Both boys, in common with two older siblings, speak indistinctly with a lisping difficulty in articulation suggestive of "baby talk."

Jimmy showed a good deal of spontaneous chatter, but was quite apt to grow almost incoherent in the rapidity with which he spouted out detail after detail and leaped from topic to topic. He showed no marked preoccupations, but reacted to the questions immediately without taking thought as to how he answered. He talked a good deal of Johnny and showed a warm affection toward him. At the same time, he tended to bring out Johnny's bad points, telling of his enuresis and hair-pulling and stating that Johnny is "the bad one at home; bad at home and bad in school." He said that Johnny wanted to be a drunken man when he grew up. He likes to play with Johnny best, and brags about how funny he is. Many of Jimmy's statements seemed to be made for effect and with a desire to produce a laugh as he would immediately contradict himself. His general attitudes and behavior seemed quite typical of an outgoing, exuberant and quite usual six-year-old boy. He reacted without thinking and was responsive to environmental changes. In one interview when the examiner was weary, Jimmy reflected the subdued and more quiet atmosphere. Jimmy in his drawing drew a watch which he then cut out and pinned to himself, strutting around with evident pleasure at his self-adornment.

Johnny in the interview presented a quite different picture from that of his brother. He was friendly and cheerful but not so spontaneous. Attention was difficult to hold; he would be distracted by extraneous noises, but more often by passing thoughts of his own which would result in a seemingly irrelevant answer. He was thoughtful in answering questions and seemed to weigh his answers. His interviews came after Jimmy's and he seemed to feel that he should have everything that his brother had had; making sure that he sat in the same chair, had his words written down, drew pictures, and took home a pencil as a gift. His attitude toward the members of the family was somewhat different from Jimmy's. He definitely prefers his mother and sister whereas Jimmy prefers the "toughest" brother to the sister whom he soundly denounces as being dumb. Johnny is quite found of his twin brother, but does belittle Jimmy's ideas of wanting to grow up twice; he himself would prefer to keep on growing until he became a giant. He did tell of a dream which he was careful to point out was "make believe" in which an old witch hits Jimmy. Johnny talks of most of his difficulties quite freely, but is hesitant about mentioning the hair-pulling. He shows a definite tendency toward self-evaluation and self-criticism which is not present in his brother's output. He also shows good imaginative ability and reveals in his stories evidence of a rich fantasy life although he will not express these when questioned directly. His drawings are rather well done and are not, as were Jimmy's, copies of something he sees, but rather a product of his own imagination.

Attitudes of both children toward the clinic were those of its being a pleasurable experience with the exception of having to take their clothes off. Jimmy, however, tended to protest that he liked Dr. McGraw better than Johnny, although both felt sure she had no preferences. In the clinic situation it would seem that Johnny has the greater security and tends to be the leader and to boss Jimmy about. His reactions at the birth of Dr. McGraw's child would tend to bear out his need to be wanted there.

In conclusion one might say that the boys present quite different pictures. Johnny exhibits in the home certain evidences of tensions; nail-biting, motor restlessness, persistent thumb-sucking as well as enuresis. The last symptom, however, is difficult to evaulate because of its prevalence in the family, and it is also present to a lesser degree in Jimmy. These symptoms could well come out of Johnny's lessened security in the home situation where his brother has been preferred and given more attention, and earlier, a greater amount of affectional demonstration. It would not seem that the "conditioning" in the clinic had much to do with these. The restlessness, nail-biting, and sucking do not occur in the clinic setting, and the boy has great security in his relationship with the clinic personnel, especially Dr. McGraw. I should not feel that undue pressure for success in various accomplishments was a factor in this as the boy enjoyed his work. This latter, however, would seem of importance in giving Johnny his attitude of evaluating and looking over a situation.

Jimmy is the outgoing, helter-skelter type of child who lives for the moment. Johnny is the more serious, thoughtful and contemplative youngster who looks to the consequences before he acts. Jimmy reacts mostly to external stimuli, Johnny as a result of his more active inner life reacts in a less direct manner to external stimuli and frequently gives the impression of preoccupation. Johnny gives the impression of being capable of weathering more serious environmental difficulties without blowing up because of his greater capacity for working things through; but once upset, he would respond more slowly to efforts for readjustment. Neither child seems to have suffered from the experimental study. Both make adequate social adjustments although in different fashions. The differences in their personalities may well be largely constitutionally determined and not entirely the result of their diverse earlier experience. However, one feels that these experiences are of importance. I should feel that the home attitudes were of great importance as well as Johnny's "conditioning" and they do seem a little easier to evaluate.

Dr. Langford is in a peculiarly favorable position to make these interpretations since he knew the children during their experimental period and has had occasion to see them from time to time around the laboratory in addition to the specified psychiatric interviews. According to the writer's observations, outstanding personality differences are Johnny's stoical and rather philosophical attitude in disagreeable situations, and his escape by indulging in his own fantasies. Jimmy is more loquacious, is more conscious of, and bids for, the approval of his audience. Consequently, he is more appealing to the casual observer. Since infancy he has looked and played the part of a clown. He is rather happy-go-lucky and reflects his home environment more directly. Johnny employs a more subtle method of revealing his feelings. It appears that the less frequent contacts with the laboratory after the age of two years have been a handicap to Johnny. Apparently having heard that younger twins came here, he spontaneously inquired "Dr. McGraw, why did you need another baby when you sent me home?" When they left here, Jimmy went to a home admittedly more favorable to him; he also had less of an adjustment to make. In some ways Johnny's experience here was poor preparation to meet the rough and tumble of a large family life. He had learned in many ways "to take it" and the reports indicate that he has had more than his share of taking. At home and at school Jimmy is rather the bully, often casting aspersions on Johnny and threatening his confidence. For all that, the affection between the two boys is warm. Despite the amount of public notice they have received, they are remarkably free from self-consciousness.

To evaluate the extent to which these personality differences are constitutional, or determined by their early experimental experiences or their later home environment, is beyond achievement at the present time and will probably remain beyond the scope of actual determination for all time. However, their experiences and behavior have been studied and chronicled as to detail in a way which is not obtainable for the average school child. If these reports and opinions serve a useful purpose in guiding the educational careers of these two children, they may at the same time be of even greater significance in showing the extent to which knowledge of early childhood experiences is of educational and psychiatric value in the adjustments of the adolescent or adult.

INTELLECTUAL AND PHYSICAL DEVELOPMENT

Both Johnny and Jimmy have consistently rated within the normal range on standarized intelligence tests. The Rorschach test indicates that Johnny is somewhat accelerated in intelligence. This paper is not the occasion to discuss the correlation of the Rorschach with the Stanford-Binet, the Minnesota, or other well-known intelligence scales. It may be that the Rorschach taps a quality of intellectual processes which is not measured by the items included in these various tests.

There is one fairly common opinion among the laity at least which might at this occasion be corrected. Some have labored under the belief that special training during the first few years of a child's life will in some way raise his general intellectual endowment. It has been previously pointed out that the effect of training or exercise as applied in these studies was highly specific to those activities in which the child received daily practice. Certainly, whatever are the factors measured by standarized intelligence tests, they have not been appreciably altered by the different experimental experiences to which the twins were subjected.

There is one aspect of mental development which deserves mention, although it has no direct bearing upon the training program. In parlor discourse the earliest individual memory is a common topic. Psychologists are usually skeptical of reports of direct memories of experiences which have occurred during infancy. The usual explanation is that the person had heard the experience referred to and therefore could not distinguish between direct and verbal memories of it. Because of the peculiar circumstances of the twins' first two years, it is possible to know precisely whether reference had been made to certain incidents in their early experiences. There are several instances in Johnny's behavior indicating remote memory. When Johnny was sixteen and seventeen months old, he would skate through the tunnel joining Babies Hospital and Neurological Institute as he journeyed to the swimming pool in Bard Hall. These journeys through the tunnel were terminated when he was seventeen months old, and he never entered the tunnel again until he was forty-three months old, that is, 26 months later. On this occasion he was walking with three other children. As he entered the tunnel his eyes widened and he suddenly remarked, with a sweeping gesture, "This is where we go skating." This recall is interesting beyond the mere indication of memory for early experiences. At the time the tunnel journeys were ended Johnny had no verbal means of expressing the act of skating. He recognized skates by name, but he had no word in his own vocabulary to indicate them. Here his remote memory is entirely integrated with verbal expression which was acquired at a later date. A year later, when he was four and one-half years old, Johnny was again escorted to the tunnel. On this occasion he asked, "Where's the bathroom?" When told he would have to wait until he returned to the laboratory, he replied, "Oh, there's one way, way down there," pointing in the direction of Neurological Institute. It was then recalled that when Johnny was seventeen months old he was being trained in toilet habits, and as soon as he arrived at Neurological Institute during those days, he was rushed to the toilet. Another instance illustrates his incorporating into his vocabulary recollection of an experience which occurred before he had language facilities for expressing such experiences. Johnny and Jimmy were just two years old when they entered the elevator of an apartment house alone and Johnny pulled the lever, sending the elevator to the basement. The occasion was subsequently never referred to, and other persons who have been associated with the children did not even know about it. More than three and one-half years later, as Johnny sat somewhat meditatively watching the dial indicate the floors of the elevator in Babies Hospital, he quietly remarked, "Dr. McGraw, remember when Jimmy and me went in your house in the elevator and it went down boom!" These incidents are cited because they are comparable to citations of many parents in reporting the early memory of their children; and because of the peculiarity of experiences to which the twins were exposed during their first two years, it is possible to know definitely whether or not subsequent reference had been made to the situations. If the direct memory of experiences during the first two years of life are as indelible as these incidents indicate, then the weight attributed to influences imposed upon the infant and young child gains significance.

There is nothing of experimental significance in the physical history of the two children. Although Johnny was smaller during the first seven months of his life, he began at about eight months to maintain a superior weight gain as compared to that of Jimmy. This advantage in body weight he has sustained during the four years they have been at home. While Johnny began to show this superiority in weight gain about the same time as he showed improvement in motor performances, there is no claim of a direct relationship between the two. Their health records compare favorably with those of other children of their socio-economic level, and their value to this study is only in so far as they indicate no serious sequelae of their experimental experiences during their first two years of life.

SUMMARY

Studies of the performances of Johnny and Jimmy in particular laboratory situations indicate that the amount of retention of a motor performance, once the factor of repetition has been reduced or abandoned, is contingent upon the state of fixity the activity had attained at the time the practice factor was withdrawn. Activities which have attained a high degree of integration may be appreciably altered if the body mechanisms are so modified through growth as to introduce new structures or elements into the situation. The natural and gradual maturing of emotional or attitudinal factors seems to influence appreciably the somatic or motor performance in particular activities.

In general endowment the two children have consistently fallen within the normal range as measured by the standarized intelligence tests. There is no reason to believe that exercise in special activities will accelerate mental functions as measured on standarized scales. There seems to be a superiority of general muscular coordinations on the part of Johnny, who received the longer and more intensive practice in motor activities. In general personality makeup, Johnny also appears to be more complex. There is no way to evaluate the extent to which their early experimental experiences operated in determining their respective personality components.

The two boys today present the picture of lively, normal six-year-old children who show no deleterious sequelae of the different regimes to which they were subjected during the first two years of their lives.

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Five—Training in Vocal Ability

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This study describes one type of early training that is retained for some months and possibly is retained for years. This is training to produce vocally musical notes and intervals. Although much more research is needed, it would appear that some types of vocal training can profitably be begun at three years of age. It would seem that notes and intervals, like words, become relatively permanent items in the child's vocal repertoire.

The purpose in this experiment was to find the extent to which three-year-old children could be taught to sing tones and intervals not already included within their vocal range. Practice and control groups were employed. The experiment as a whole included a study of such varied factors as ability to sing tones, intervals, and songs, the effect of training on this ability, and the range and content of children's spontaneous vocalizations during their free play. In the present connection only the effects of training on ability to reproduce tones and intervals will be discussed.

PROCEDURES

The first step in the study was to test each child's ability to reproduce pitch, that is, to sing separate tones, such as middle C, D, E, and so on, and his ability to reproduce intervals, that is, to sing a unit of two successive tones of different pitch. Initial tests of this sort were given to 48 children, from thirty-one to forty-eight months of age at the beginning of the study. The subjects were members of a nursery school and three day nurseries.

The initial pitch tests consisted of the following 11 notes, beginning with middle C: C D E F G A B C D E F. The initial interval test included 12 intervals as follows: Ascending and descending major and minor seconds and thirds, perfect fourths, and perfect fifths.

The procedure during the tests was briefly as follows: The experimenter had at hand a pitch pipe, a small xylophone, and a psaltery. A given note was sung by the experimenter and sounded on one of the instruments. The child in turn was directed to reproduce the tone while it was being sounded. Various techniques were used to acquaint the child with what was expected of him and to induce him to sing. The children soon learned to respond in the desired manner.

In the pitch tests each tone was presented separately. The experimenter kept singing and sounding the tone to prompt the child's efforts. If the child reproduced the tone correctly, another item was presented. If he made no response or made an unsuccessful attempt, he was given a maximum of eight additional presentations of the same item or as many as could be given without provoking loss of interest and attention. If the child reproduced the tone, he was credited with a "hit"; if he tried and failed, with an "attempt"; if he made no response, with a "no attempt." The number of presentations of each tone was also recorded. Each of the 11 notes of the pitch test was presented in this manner.

The pitch test, covering all 11 notes, was given three times on three different days. After the conclusion of this test the interval test was administered. In this test the procedure was similar to that just described. The two notes constituting each interval were presented and a record was made of the child's response. The interval test was given twice on different days. To complete the initial tests accordingly, it was necessary to visit each child on five separate days.

The need of a valid means of scoring the child's response presented an immediate problem. The use of a tonoscope or other form of mechanical device entails practical difficulties in work with children so young, while any other procedure is open to subjective errors. After much preliminary work, however, it was decided to try the method of having the experimenter judge whether or not the child's vocalization correctly reproduced the tone which was being sounded. With the use of this method no response was put down as correct unless in the experimenter's judgment the child gave a clear-cut reproduction of the required tone. A description of the methods employed to determine the reliability of the experimenter's judgments follows:

The experimenter was first tested by another accredited musician. The tester sang notes at random, taking her cue from a piano beyond the hearing of the experimenter. The experimenter, in turn, was required to identify, with the aid of a pitch pipe, and to record in writing, each note that was being sung. When the test was completed, an item by item comparison was made of the tester's record of the tones which had been sung (in the exact order) and the record of the experimenter's judgments. On three repetitions of this test the experimenter made a perfect score each time.

To obtain an even more significant test of the experimenter's judgments two other musically trained observers were introduced on two occasions during actual tests of the children. The experimenter and the persons thus introduced took simultaneous but independent records of each child's performance. On one occasion the performance of 20 children who were given the entire pitch and interval tests was thus judged by the experimenter and a second examiner. An item by item comparison of the records showed an agreement of over 96 per cent in the case of pitch (in the total of 220 notes to be judged when each of 20 children was tested on 11 notes there was disagreement on only 7 notes.) The two examiners showed an agreement of 95 per cent in their judgments on the interval tests of the 20 children. On a later occasion another musician, who, like the examiner mentioned above, possessed what is known as "absolute pitch," took independent records during the pitch tests of 10 children. An item by item comparison of recorded judgments in this case showed an agreement of 96 per cent. Out of a total of 110 responses, two were credited as correct reproductions by the experimenter, and as incorrect by the second examiner; the examiner, on the other hand, had given credit for two responses which were judged incorrect by the experimenter.

The results obtained in the projects above indicate that the testing method used in this experiment yielded a high degree of reliability within the limits of the procedure employed, though it must be said that unanimity among judges is not a substitute for mechanical recording. On the other hand, it can be seen that the procedure here used corresponds to the practical test which an individual must face either in connection with vocal training at school or in auditions of a professional character.

The coefficients of reliability of the tests, as measured by correlating the scores in successive tests at the beginning of the experiment and at later stages, ranged from .54 ± .11 to .95 ± .02, with a median of .80 ± .06.

Separate records were kept of each child's performance on each item of the pitch and interval tests on each occasion that they were administered. In computing the initial scores, the child was given one credit for each item correctly sung during one or all of the repetitions of the test. This permitted a maximum score of 11 and 12 respectively on the pitch and interval tests. Nineteen children enrolled in a nursery school were designated as the practice group. The records of 29 additional children who were given the initial tests were examined to find control subjects as nearly equivalent as possible to the practice group with respect to scores in pitch, intervals, and age.

Following the initial tests, training of the practice group was begun. The training consisted of drill in the singing of songs containing notes within and beyond the child's pitch range, and periodic repetition of the pitch and interval tests with emphasis on items which the child had hitherto failed to reproduce. The children were given training in groups of two and sometimes three. The policy of grouping a relatively superior with a less competent child was used during training. The children were rotated so that the same two children did not sing together on successive days. Each training period was designed to last 10 minutes; at the beginning of the study a shorter period was usually found to be expedient, while during later stages a lengthened period was employed to compensate for earlier abbreviated sessions and to bring the average time to approximately 10 minutes. The experimenter worked with the children four mornings each week. Each child received two or, when time would permit, three training periods on different days each week. If a child was absent from school, his training was brought up to date by extra practice periods on his return.

Training was continued over a period of approximately six months, with loss of time during vacations and holidays, and with some slowing up of the procedure to make room for periodic retests. Since the tests, the training series in reproduction of tones and intervals, and drill in the singing of songs all provided exercise in the singing of tones and intervals, each session devoted to the child was counted as a training period. During the course of the experiment each child received 40 training periods, with an average duration of 10 minutes.

After training had been in progress for some time it became apparent that the range of tones and intervals included in the initial tests could not accommodate the effects of practice. Several children soon learned to sing all the items of the initial tests. To provide for this condition additional tones and intervals were introduced. The pitch test was augmented to include the 11 original notes plus G, A, B below middle C, and G, A, B, and C two octaves above middle C, a total of 18 notes. To the intervals' test was added the following intervals: Ascending and descending major and minor sixths and sevenths, and octave, thus providing a total of 22 intervals.

The response of the children to the tests and to the training was decidedly favorable. This held true in the case of both the practice group and the controls. After the first session of the initial tests the children responded readily to another invitation to "come and have music." On only a few occasions would a child appear reluctant to leave the activity in which he was engaged when the invitation was given. Requests for another turn or for a prolongation of the music period were heard more frequently than requests to be relieved. At no time was a child induced to participate against his apparent wishes.

The initial tests of the practice group were completed in December, 1930. Tests of control subjects were begun in December but were not completed until early in 1931. To compensate, in part, for this discrepancy in the time of completion of the initial tests of practice and control groups, the latter group was not given the end tests (after training) until after the tests of the practice group had been completed.

The first retest during training occurred in January, immediately after the Christmas holidays and before practice had been given other than that entailed by the initial tests. Retests were made again in February and in March. At the latter time the additional notes and intervals of the extended test (described above) were first introduced. Training came to an end in May. At this time each child of the practice group was tested twice on reproduction of pitch, twice on reproduction of intervals. The retests of the control subjects followed. The entire pitch test was administered to the controls on three separate occasions. The intervals' test was likewise administered three times. The provision of three separate tests of pitch and intervals in the case of the controls as compared with two in the case of the practice group gave the former children the advantage of an extra opportunity to reaccustom themselves to the procedure and also gave the experimenter additional time to reestablish rapport with the subjects. In scoring the final tests the procedure followed in scoring the initial tests was used.

Each item correctly reproduced, whether on each test or only once during the test series, was given a count of one. The maximum score on the final performance, including the items of the extended tests was, accordingly, 18 in the case of pitch, 22 in the case of intervals.

The summer vacation immediately followed the completion of the retests at the end of training. When the schools reopened again in the fall, the children of the two groups were once more tested. These tests were applied during the month of October, more than 10 months after the beginning of the investigation and 4 months after the end of the training period. On this occasion the pitch and interval tests were repeated three times with each subject and were scored as before.

As was mentioned above, the practice group originally included 19 children. One child was withdrawn from school during the course of training. The remaining 18 children completed the training series. Before the completion of the study several children originally included in the control group had become unavailable through illness and removal. The losses occurring in this way disturbed the original alignment of controls matched with practice subjects. As is shown in Table I, the control subjects who could be located for retests had an average age of about two months above the practice group average. Retests were made of children who had initial pitch records sufficiently similar to the practice group to serve as control subjects. Owing to unavoidable circumstances, the interval tests of two of these control children could not be completed; as a result only 16 pairs of children are indicated in the table of results of the interval tests. When the final retests were made after the summer vacation, it was possible to obtain records of 15 of the original members of the practice group and 14 of the members of the control group in the case of pitch, 13 in the case of intervals.

RESULTS

Table I gives the average scores of the practice and control groups when measured at the beginning of the study, at the end of training, and again after the elapse of a further period without training. Separate entries are made of performance on the original tests and on the augmented tests which were introduced after it became apparent during the course of training that the practice group could master more than was originally assigned.

As was previously mentioned, all the subjects were not available for the retests given in October following a period of no training for either group. In view of this condition, two presentations of the results are given: First, the scores of all the children who were in-

TABLE I

Practice and Control Group Scores in Pitch and Intervals. Separate Entries of Average Scores (Number of Items Reproduced) of Original Tests and of Extended Tests Introduced During Course of Training. Age in Months as of January 1, 1931.

A second entry shows the average of 14 practice children matched as nearly as possible with the 14 controls who were available for the final pitch retests, and 13 similar pairs in the case of the interval tests. Average deviations are given in parentheses.

PITCH: ALL SUBJECTS

			Initial Tests (11 notes)	*Retests after 40 10-minute Practice Periods* (May '31)			*After Period of No Training* (Oct. '31)	
	N	C. A.	(Dec. to Jan. '31)	Initial (11)	Extended (18)	N	Initial	Extended
Practice Group ...	18	38.2 (3.7)	4.22 (2.44)	10.72 (0.11)	15.5 (1.78)	15	10.26 (0.76)	16.86 (1.0)
Control Group ...	18	40.1 (3.9)	4.22 (2.7)	6.44 (2.89)	8.0 (3.22)	14	7.0 (3.71)	10.21 (4.8)

INTERVALS: ALL SUBJECTS

			Initial Tests (12 intervals)	*Retests after Training*			*After Period of No Training, as Above*	
	N	C.A.	(Dec. to Jan. '31)	Initial (12)	Extended (22)	N	Initial (12)	Extended (22)
Practice Group ...	16	38.7 (2.9)	4.25 (3.03)	11.5 (0.67)	17.0 (4.25)	15	11.4 (0.96)	19.0 (3.6)
Control Group ...	16	40.6 (3.7)	4.31 (2.89)	8.0 (2.5)	10.18 (4.26)	13	7.38 (3.59)	9.84 (3.8)
PITCH (*14 Pairs*)								
Practice Group ...	14	39.14 (3.2)	4.7 (3.0)	10.8 (2.5)	16.0 (1.6)		10.9 (.15)	16.8 (1.5)
Control Group ...	14	40.14 (4.0)	4.5 (3.2)	6.21 (2.8)	7.9 (3.2)		7.0 (3.4)	10.2 (4.8)
INTERVALS (*13 Pairs*)								
Practice Group ...	13	39.5 (3.1)	4.38 (3.5)	11.76 (0.48)	19.0 (2.9)		11.3 (1.1)	18.69 (3.9)
Control Group ...	13	42.0 (3.7)	4.46 (3.1)	7.61 (2.8)	9.0 (3.5)		7.4 (3.6)	9.8 (6.26)

cluded in the experiment, with a parenthetic notation of the number of subjects included in the final retests; and second, a separate statement of the averages obtained when the 14 control subjects who were available for the final pitch retests and the 13 available for the interval retests are paired, as nearly as possible, on the basis of initial records, with a similar number of children of the practice group. The two entries are based on identical data, save that the first includes the records of a greater number of subjects.

TABLE II

AVERAGE SCORES (AND AVERAGE DEVIATIONS IN PARENTHESES) ON THE PITCH AND INTERVAL TESTS WHEN REPEATED AT VARIOUS TIMES DURING TRAINING

Separate averages are given for the extended tests used during the later stages of training.

PITCH

11 ascending notes from middle C to F			18 notes from G (below middle C) to C (2 octaves above middle C)		
Time of Test	*N*	*Average No. of Notes Correctly Sung*	*Time of Test*	*N*	*Average No. of Notes Correctly Sung*
December 1930	18	4.22 (2.44)			
January 1931	18	4.56 (2.90)			
February 1931	18	6.50 (3.33)			
March 1931	18	8.78 (2.00)	March	18	10.67 (2.78)
May 1931	18	10.72 (0.11)	May	18	15.50 (1.78)
October * 1931	15	10.26 (0.76)	October *	16	16.86 (1.00)

* After no training since May.

INTERVALS

Ascending and descending major and minor seconds and thirds, perfect fourths and fifths. Total: 12 intervals			12 intervals in left column plus major and minor sevenths and octave. Total: 22 intervals		
Time of Test	*N*	*Average No. Intervals Correctly Sung*	*Time of Test*	*N*	*Average No. Intervals Correctly Sung*
December 1930	18	3.94 (3.05)			
January 1931	18	4.33 (2.89)			
February 1931	18	8.05 (2.61)			
March 1931	18	9.33 (2.33)	March	18	11.11 (3.44)
May 1931	18	11.56 (1.00)	May	18	17.44 (4.0)
October * 1931	15	11.4 (0.96)	October *	15	19.0 (3.6)

* After no training since May.

The results shown in Table I indicate a substantial and statistically reliable advantage in favor of the practice group at the end of training. When retests were made after four months of no training the practice group was still well in the lead, both in pitch and intervals.

The periodic tests which were made during training present a general picture of the rate of improvement even though tests were not given at equal intervals. Table II shows the average scores obtained at various stages of practice. In the case of the interval tests, averages are given for 18 subjects, as compared with only 16 in Table I; the presentation of results of only 16 cases in the former case, as is previously explained, is due to lack of a sufficient number of control subjects.

The results in Table II show a steady increase in performance. As the average score increases, the relative variability tends to decline. This decline in variability, which was particularly marked on the final tests, largely results from the fact that the tests limited the maximum score that could be obtained. Several children correctly reproduced all the 18 notes and 22 intervals that were presented. (It is of interest, incidentally, that when 29 parents of the children here represented, 15 mothers and 14 fathers, were tested for pitch with no limit to the score, the 29 parents sang an average of about 20 notes as compared with an average of 15.5 notes sung by the three-year-old children when tested on a limited range of 18 notes.)

The benefit derived from training in this experiment was more pronounced and decidedly more permanent, relative to the performance of the controls, than was anticipated by the investigator at the beginning of the study.

It is possible that the average would be different if mechanical devices rather than personal judgments had been used in the administration of the tests. The investigator is convinced, however, that even though different testing methods might have modified the averages, the general results would have been the same. This view is supported both by the measures of reliability which were obtained by the use of additional observers in this and a later study and by observations of the changes in individual children. Children who at the beginning of the study were unmistakably limited in their vocal range later essayed high and low tones quite beyond their original performance.

The results of a subsequent study in which the present experimenter's judgments were checked by three additional judges, all of whom took simultaneous and independent records during tests of 23 children, further strengthen the view that the results here set forth represent in general an authoritative and practical evaluation of the conditions which were being studied.

The figures in Table I show that during the summer vacation the discrepancy between the scores of the practice and control children was reduced to some extent. The former group maintained its score pretty well, while the latter improved upon some previous averages. The leveling tendency is rather slight, however, and the practice children still retain a distinct advantage.

There arises the question whether the improvement represents a fundamental change or merely the accelerated appearance of abilities which normally would develop at a later time. It is undoubtedly true that sooner or later, in the course of further experience and growth, most of the group would be able to achieve the average scores shown in the present results. Whether continued intensive training at this time might eventually raise the child's performance beyond anything that could be attained if training were postponed until later years, the data do not tell. Some observations made during the study do, however, offer certain suggestions.

The singing of some children at the beginning of the study was definitely confined to a restricted tonal area. One child's voice, for instance, was placed so low, both in singing and in daily speech, as to cause her mother some concern. As training progressed this child (as well as others similarly situated at the start) was able not only to essay the higher pitch range but also to reproduce specific high tones. During the course of training it sometimes happened that a child was able to sing a group of low notes and a group of high notes without being able to manage the notes between. The child's behavior had many of the earmarks of trial-and-error behavior. Through encouragement and example he was led to attempt tonal areas quite beyond his original repertory. Further vocal manipulation was necessary before he could reproduce a particular tone in this area. With additional exercise he was able to sing the particular tone without making a sliding approach. Still more vocal experimentation and exercise were required to bridge the gap between the high and low tones.

Observations of individual children indicated that the particular pitch range to which a child confined his singing and his speech may sometimes be largely a fortuitous matter rather than an expression of native capacity; further, that a child whose voice is persistently low or high or, to all appearances, capable only of monotones, may show radical improvement when patient training is given. It is possible that an early tendency to use only a limited tonal repertory may become intrenched as a fixed habit. If this is true, the value of early training is clear. According to this hypothesis, any attempt to increase the pitch range in later years may be rendered difficult because of a persistent habit of using only certain tones, somewhat in the same way that old speech habits interfere with the mastery of the accents of a foreign language.

The above statements do not imply that training at an early age increases capacity, on the one hand, or that lack of exercise causes deterioration of capacity through disuse. The view here set forth, partly on the basis of the quantitative results and of observations of individual children and partly on the basis of speculative premises, may be rephrased and summarized as follows: A child's tonal range often represents only a part of what he actually can produce, the particular tones which he employs may reflect the beginnings of vocal habit, originating in a more or less fortuitous manner; the development of proficiency in the use of a wide tonal range is similar in many ways to the acquisition of a new skill, requiring vocal manipulation and a trial-and-error approach to the production of new tones; by means of early training a child may be enabled to sing tones quite beyond his accustomed range; it is possible that early training may give the child a lasting advantage over other children, with similar original endowments, whose efforts to improve upon their accustomed ranges are not begun until a later age when old habits interfere with the establishment of new skill in the use of the voice.

Six—Retention of Visual Imagery by the Blind

JOSEPH JASTROW

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Disease and injury sometimes perform valuable if cruel experiments. Blindness creates situations whose results are of considerable interest. Some children are blind from birth. Others, from various causes, lose their sight at one, two, and more years of age. How old must a child be when his sight is lost in order to be able to retain visual imagery? This is the problem which Jastrow undertakes to investigate.

It may be pointed out that the results lend no support to the notion that the early experiences of the child are well retained.

A fact of prime importance regarding the development of visual imagery is the age at which it can continue to function without further visual experience. If we accept as the test of the existence of visual imagery its automatic excitation in dreams, the question can be answered by determining the age of the onset of blindness which divides those who do not from those who still retain in their dream-life the images derived from the world of sight. The data that enable me to answer this question were gathered at institutions for the blind in Philadelphia and Baltimore. Nearly 200 persons of both sexes were personally examined, and their answers to quite a long series of questions recorded. All dates and ages were verified by the register of the institutions, and the degree of sight was tested.

Beginning with cases of *total* blindness (including under this head those upon whom light has simply a general subjective "heat-effect," enabling them to distinguish between night and day, between shade and sunshine, but inducing little or no tendency to project the cause of the sensation into the external world), I find on my list 58 such cases. Of these, 32 became blind before the completion of their fifth year, and *not one* of this group of 32 sees in dreams. Six became blind between the fifth and the seventh year: Of these, four have dreams of seeing, but two of them do so seldom and with some vagueness; while two never dream of seeing at all. Of 20 persons who became blind after their seventh year *all* have "dream-vision"—as I shall term the faculty of seeing in dreams. *The period from the fifth to the seventh year is thus indicated as the critical one.* Before the age of five, if the visual center of the brain is deprived of impulses from the eye, it apparently ceases to function. If blindness occurs between the fifth and the seventh years, the preservation of the visualizing power depends on the degree of development of the individual. If the faculty is retained, it is neither stable nor pronounced. If sight is lost after the seventh year, the sight centers of the brain can, in spite of the loss, maintain their function; and the dreams of such an individual may be hardly distinguishable from those of a seeing person.

With regard to the *partially* blind, the question most analogous to the persistence of dream-vision after total blindness is whether or not the dream-vision is brighter and clearer than that of waking life; whether the sight center maintains the full normal power or whether the partial loss of sight has essentially altered and replaced it. To this rather difficult question I have fewer and less satisfactory answers than to the former inquiry; but the evidence is perfectly in accord with the previous conclusions. Of 23 who describe their dream-vision as *only as clear* as waking sight, all became blind *not later* than the close of their *fifth year*; while of 24 whose dream-vision is more or less markedly *clearer* than their partial sight, *all* lost their full sight *not earlier* than their *sixth year*. The age that marks off those to whom total blindness carries with it the loss of dream-vision from those whose dream-vision continues is thus the age at which the visual cortex has reached a sufficient stage of development to enable it to maintain its full function, when partially or totally deprived of retinal stimulation. The same age is also assigned by some authorities as the limiting age at which deafness will cause muteness (unless special pains be taken to prevent it); while later the vocal organs, though trained to action by the ear, can perform their duties without the teacher's aid. This, too, is assigned as the earliest age at which we have a remembrance of ourselves. This last statement I am able to test by 100 answers, collected among these blind persons, to the question, "What is your earliest remembrance of yourself?" The average age to which these memories go back is 5.2 years; 79 instances being included between the third and the sixth years.

Seven—An Experimental Study of Early Childhood Memory

HAROLD E. BURTT

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Few experiments cover a period of 18 years. Here is a truly heroic one which does.

Can one learn Greek in the cradle and retain it in college? This might be said to be the problem which is investigated. But of course the results are expected to have a wider applicability.

INTRODUCTION

There have been numerous reports as well as informal collection of data regarding the earliest period in life which a person can remember. The data usually take the form of memory for some particular event which can later be placed chronologically by other criteria. The obvious difficulty is that actual memory for the event may be confused with memory of accounts of the event which have been related by the child's parents. A crucial experiment necessitates the use of material which can be checked objectively in such a way as to eliminate all possibility of others coaching the subject. The general program of the present experiment was to present nonsense material to the subject in infancy and early childhood and later conduct learning experiments with this material as contrasted with other material of similar character which had not been presented in infancy. If the material which had been presented at the outset was relearned more readily than the new material was learned this would indicate some memory effect produced by the first presentation.

MATERIAL

Passages from Sophocles' *Oedipus Tyrannus* in the original Greek were used at material. This was tantamount to nonsense material for the subject. Furthermore, no members of the family or relatives who might overhear the experiment had any knowledge of Greek so that there would be little opportunity for anyone subsequently to coach the subject deliberately or inadvertently. The material being in the same meter and dialogue form was presumably fairly uniform in difficulty throughout. Selections were taken from scattered points throughout the play. No choruses were used, but merely dialogue portions, all in iambic hexameter. Each selection involved approximately 20 lines, 240 syllables. Occasionally there were a few syllables more or less than that due to the inclusion of more than two syllables in a measure. Moreover, a slightly longer cue was necessary at the beginning of some selections because several of them were rather similar in the first words. In one or two instances slight variations in length were made in order to preserve the unity of the selection. On the average, however, the actual number of syllables learned in the original passages and the new passages differed by only three syllables—less than 1 per cent.

METHOD

The subject was a boy with an IQ of approximately 130 based on the average of periodic Binet examination. Twenty-one passages similar to those above described were selected for the original presentation. Beginning at the age of 15 months three of these passages were read once daily to the subject for a period of three months, a total of 90 repetitions. The reading was at as uniform a rate as possible, approximately 2 seconds per line. At the age of 18 months these three passages were dropped and three more read daily for three months. This procedure was continued until the subject was three years old.

The subject was always kept in a situation as conducive as possible to maximal attention to the reading. No toys or playthings were permitted and no activity such as running about was allowed, although minor squirming and restlessness could not be controlled. In the later portions of the original reading, of course, the subject began to take the procedure as a matter of course and called it his "Greek lesson."

LEARNING AT EIGHT AND ONE-HALF YEARS

After the subject reached the age of three years, the entire matter was dropped until he was eight years and six months of age. No one else had heard enough of the material to be able to recite any of it and the experimenter of course did not mention it. At the age of eight years and six months the experiment on relearning began. For this purpose the 7 selections noted in Table I—one from each age level—were used and three new ones of the same average length and meter and presumably of approximately the same difficulty. The nature of the experiment was explained to the subject and his cooperation was adequate. He was not told, however, which passages were new and which were old, so that there would be no difference in motivation. He did not have any correct suspicion on the matter as evinced by the fact that when the experiment was over and the results were explained he showed considerable surprise at the information as to which passages were new and which were old.

TABLE I

Age in Months for Original Reading	*Selection*	*Repetitions till All Words Anticipated*	*Repetitions till Recited Entire*
15-18	III	361	382
18-21	VI	250	253
21-24	IX	376	385
24-27	XII	355	379
27-30	XV	304	328
30-33	XVIII	217	226
33-36	XXI	259	265
Av. of all		303	317
Av. of 3 early		329	340
Av. of 4 later		284	299
Control	*A*	409	409
	B	445	451
	C	436	445
Av. control		430	435

The order of presenting the 10 selections in each trial was varied systematically. The first trial was in the order A, B, C, XXI, XVIII, XV, XII, IX, VI, III. The next was B, C, XXI, XVIII, XV, XII, IX, VI, III, A. The next was C, XXI, XVIII, XV, XII, IX, VI, III, A, B, and so on. Thus, in each succeeding trial the selection which had been at the first of the preceding list was now at the last of the new list. Ten trials completed this cycle whereupon it was repeated and this procedure gave each selection an even chance of any advantages which might be due to primacy or recency or any disadvantages of retroactive inhibition or similar processes. The trials were not run on an absolutely regular schedule. It was desired, as far as possible, to have them once daily but there were unavoidable interruptions. On occasion two trials would be given daily when time was available, but on every trial the 10 selections were always given with the usual 15-second pause between them.

For the first 17 trials the experimenter merely read the selections in the same fashion they had been read at the outset. Beginning with the eighteenth trial the prompting method was adopted as follows: The selection was read very slowly and the subject supplied any words which he could at the proper point. As the experiment progressed the subject would be reciting more and more words and phrases and the experimenter would merely be supplying the missing parts. On the nineteenth trial it was realized that a tremendous amount of time would be consumed if the prompting method was used on every individual trial and that probably very slight differences would be found from one trial to the next anyway. Consequently, the procedure was modified so that the experimenter read all the selections for two trials at the standard rate of 2 seconds per line and then the third trial was devoted to the prompting method. This arrangement was maintained until the end of the experiment so that trials 19, 22, 25, 28, etc., were prompting trials and the learning data were tabulated on the basis of these trials.

As the words were learned they were underlined in the book and the date noted on which they had been anticipated correctly. This procedure was continued until every word in the selection, with the exception of the initial cue words, had been anticipated. A record was made of the number of repetitions necessary up to this point. In most cases the subject was not at this time able to recite the entire selection without prompting and further repetition on the same schedule was made until he recited the selection from beginning to end with no prompting. This number of trials then was recorded as the final score. Due to the way the repetitions were distributed it was about 11 months until the first selection had been completely learned. It was then dropped from the schedule. It was about 16 months before the last selection had been perfected. It was possible that there was a little greater incentive toward the end of the experiment as the subject realized he was getting some of the selections out of the way. The effect of this greater incentive would be, if anything, to complete the more difficult selections earlier than they normally should have been. If any correction were to be made for this it would probably take the form of delaying the correct scores in the later trials so that the results hereinafter reported might perhaps be more significant even than they appear.

The principal results are summarized in Table I. The first two columns have already been explained. The third column gives for each selection the number of repetitions that were necessary until all the words had been correctly anticipated. As explained above, the subject might not be able to recite the passage entirely at that time. The figures in the last column give the number of repetitions necessary before he recited the entire selection without prompting. The upper block of the table with the Roman numerals involves the selections which had been presented in infancy or childhood, while the lower block gives the three new selections, A, B, and C, which were learned *de novo*.

Perhaps the clearest way to note the general tendency is to average the results of each block of the table. The results are rather striking, with 430 repetitions necessary for the new material as against 303 for the old from the first standpoint, and 435 and 317, respectively, from the second. These differences are 42 per cent and 37 per cent. It may be noted that there is no overlapping of the two distributions. The most rapid learning is for selection 18 with repititions of 217 and 226 by the two criteria of learning. The values contributing to the averages in the upper part of the table are naturally rather scattered because different amounts of memory might be expected due to the varying lapse of time since the original presentation and the age of the subject at that presentation. Nevertheless, if we disregard this fact and average the memory scores for the original selections, it appears that we do have real differences between such scores and the memory for new selections. With small samples of this sort the accepted procedure is to use Fisher's t-function. For the averages of 303 and 430 repetitions necessary until all the words had been anticipated the value of t is 3.34 which gives almost exactly 99 chances out of a 100 that the difference between the averages is a real one. For the averages in the last column, t is 2.86 which gives a probability of about 98 out of a 100 that the diferenence is real. As mentioned above, the high variability of the relearning data is to be expected anyway, because of the different conditions of the original learning. Even so, the differences between the averages are significant.

It is interesting to divide the upper portion of the table into two parts—the three selections presented earliest of all, III, VI, IX, and the remaining four. Averages of these are given. All of them, of course, are smaller than the corresponding average for the new selections, A, B, and C. The earliest selections, III, VI, and IX, required more repetitions than the later ones. All four averages differ significantly from the A, B, C averages with the possible exception of the difference between 340 and 435 in the last column, when the probability is only .90 that the difference is real. The difference between the averages for earlier and later selections, 329-284 and 340-299, are of somewhat doubtful statistical significance.

Again, if we consider the individual scores, there appears somewhat of a trend for the selections which had been presented originally at a later age to be remembered better than those presented at an earlier age. Selection VI is a definite exception to this trend but otherwise we note a general decrease in the magnitude of the entries in the table as we go down the column. If we simply rank the seven values in either column and compare those ranks with the actual order in which the rows are listed, that is, the original order of presentation, we get a correlation by rank difference squared formula of .50±.19. Again if we simply correlate the values in a given column with the age and month in which the original reading was begun, namely, 15, 18, 21, and so on, in the second column, we get a correlation of .55±.18 for repetition until all words are anticipated and a correlation of .52±.19 for repetition until the entire selection is recited. This trend conforms to what we might expect—that the selections which had been presented at the earliest age were retained least effectively.

LEARNING AT FOURTEEN YEARS

The eight-year experiment utilized only one-third (seven selections) of the available material which had been presented in infancy plus three new selections. The fourteen-year experiment employed another third of this original material plus three other new selections, and makes possible a comparison of the "saving" at these two ages.

The procedure at fourteen years was practically identical with that at eight and a half, except that the distribution of trials was a trifle more uniform. Two trials were given daily for the most part. One interruption of five days occurred at about the one hundred and fiftieth trial after several of the selections were already completed and out of the way. The entire 10 selections under comparison were always given in a trial, and their order from trial to trial was rotated systematically. For instance, if one trial was II, V, VIII, XI, XIV, XVII, XX, *D*, *E*, *F*, and next would be V, VIII, XI, XIV, XVII, XX, *D*, *E*, *F*, II. As in the earlier experiment, the 10 selections were merely read to the subject for the first 18 trials. Beginning with the nineteenth, every third trial used the prompting method in which the selection was read slowly and the subject anticipated any words or syllables which he could. These words were underlined in the text and the date noted at which they had first been anticipated correctly. This procedure was continued until every word in the selection, with the exception of the initial cue words, had been anticipated and the requisite number of trials constituted one item of score. The repetitions were continued if necessary until the subject could recite the entire selection without prompting, and the number of trials up to this point constituted the second item of score. The attitude of the subject in the present case was somewhat superior to that in the earlier experiment because he understood the scientific importance to a greater extent. Motivation seemed entirely adequate throughout. Toward the end there was a little unavoidable further motivation as certain selections were learned and dropped from further consideration and effort was made to get still others out of the way.

A variable which could not be controlled was the occasional tendency for the subject to think of words or phrases between trials. A little beyond 100 trials he began to recite phrases spontaneously when riding, walking, or swimming. According to his report at the end of the experiment, he seldom did this except when in the company of the experimenter. On this basis the occasions were not very numerous. The experimenter made no comment on such occasions. After trial 118 the subject remarked that some of the selections were nearly learned, and quoted part of selection *XX* to illustrate his point. A few days later he realized that *XX* was complete except for one group of words and he tried to quote it between trials in order to determine where he was "stuck." Subsequently, he listened intently to that part of the selection. The above is the only instance that the experimenter observed in which the conduct of the subject gave any selection an undue advantage. It is probable that this consideration between trials affected the selections about equally. When questioned at the conclusion of the experiment, the subject reported that no selections had been particularly impressive and he had no notion as to which ones were being relearned and which ones learned *de novo*.

The main results, together with some data from the eight-year experiment, by way of comparison, appear in Table II. The first column gives the age at which the original reading took place. The Roman numerals in the second column indicate the arbitrary designation of the selections that were used in the eight-year experiment. The two following columns give the number of repetitions necessary before the subject had anticipated each word in the selection and also the repetitions required in order to recite it verbatim without prompting. The remaining columns give similar data for the fourteen-year experiment. The two selections involved in a row of the table had been presented in infancy under identical conditions. The averages for certain groups of selections are given in the rows indicated. The lower portion of the table gives the data for selections which were learned *de novo–A, B,* and *C* for the eight-year experiment and *D, E,* and *F* for the fourteen-year experiment.

TABLE II

	Eight-year Experiment			*Fourteen-year Experiment*		
		Repetitions			*Repetitions*	
Age in Mos. at Original Reading	*Selection*	*Anticipate Each Word*	*Recite Entire*	*Selection*	*Anticipate Each Word*	*Recite Entire*
15–18	III	361	382	II	142	142
18–21	VI	250	253	V	133	139
21–24	IX	376	385	VIII	148	169
24–27	XII	355	379	XI	148	151
27–30	XV	304	328	XIV	145	145
30–33	XVIII	217	226	XVII	169	169
33–36	XXI	259	265	XX	127	127
Av. of all		303	317		145	149
Av. 3 early		329	340		141	150
Av. 4 later		284	299		147	148
Control	*A*	409	409	*D*	163	169
	B	445	451	*E*	145	151
	C	436	445	*F*	163	166
Av. control		430	435		157	162

The most general notion of the results may be obtained from the averages. It requires 145 repetitions before each word or syllable in the selection is anticipated in the relearning experiment, while with entirely new selections of comparable difficulty 157 repetitions are necessary. The corresponding averages, when it is a matter of actually reciting the selection without prompting, are 149 and 162. A considerable overlapping of the distributions is apparent, as some of the new selections require less repetition than some of those which are relearned. The saving averages about 8 per cent. As a small number of items contribute to each average, it is necessary to analyze the difference between averages according to the small sample theory (Fisher). In both of the above cases the probability, by this method of computation, is about 0.8 that the difference is real. Thus, from a statistical standpoint the differences cannot be considered entirely significant. It is interesting to compare the present experiment with the earlier one. In that case the saving was about 30 per cent as contrasted with the present 8 per cent, and the differences were of undoubted statistical significance. The intervening six years produced a considerable loss in retention for the material presented in infancy.

Continuing the analysis in the same way that it was made with the first set of data, the relearning is divided into two portions—Selections II, V, and VIII which were originally presented when the subject was youngest, and Selections XI, XIV, XVII, and XX which were given later in infancy. The averages appear in the rows of the table labelled "Av. three early" and "Av. four later," respectively. In the eight-year experiment the selections presented later in infancy actually required less repetition for relearning on the average. In the present instance this tendency has disappeared. Again, the seven figures in a given column may be ranked in order of magnitude and those ranks correlated with the order in which the selections were presented originally in order to discover any tendency for the later selections to be learned more readily than the earlier ones. In the eight-year experiment a correlation of .50 was obtained in this way. In the present case the correlation is zero.

Learning curves are shown in Figure 1. The abscissa indicates the trial and the ordinate the cumulative number of syllables correctly anticipated up to and inclusive of the trial. The solid curves are for eight and a half years and the dotted ones for fourteen. The upper curve in each instance is for relearning and the lower for learning *de novo.* The separation of the curves was scrutinized for its statistical significance. Typical ordinates were taken at intervals of 21 trials and the significance of the difference between the ordinates for two curves computed in accordance with small sample theory. In the fourteen-year curves, after the first few trials, these differences have probabilities greater than .99 that the difference is real. This condition exists until near the eightieth trial when the probability becomes approximately .98. Thereafter the significance of the differences

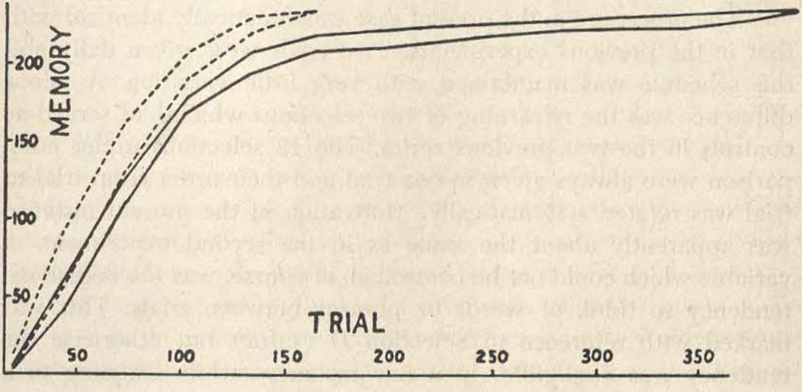

Figure 1.

decreases as the curves approach one another. In the above respect the present experiment differs from the earlier. In that case, practically none of the differences between ordinates was significant. The result is perhaps attributable to the slightly greater homogeneity of data in the present case. It appears that the selections came along together to a greater extent than they did in the original experiment.

Another difference between the present and the earlier study is the much smaller number of repetitions necessary for both relearning and learning. For example, the three new selections required 162 repetitions for a complete recital in the present case, whereas in the earlier study they required 435. This difference is due partially to the greater maturity of the subject and to better motivation. He appreciated the scientific importance of the experiment and was also aware of the fact that when he had learned a selection he would be through with it. The main interest of the study, however, was in the comparative saving rather than the absolute scores.

LEARNING AT EIGHTEEN YEARS

The procedure in the present case was practically identical with that in the previous experiments. Two trials were given daily and this schedule was maintained with very little variation. A minor difference was the relearning of two selections which had served as controls in the two previous series. The 12 selections under comparison were always given in one trial and their order from trial to trial was rotated systematically. Motivation in the present instance was apparently about the same as in the second experiment. A variable which could not be controlled, of course, was the occasional tendency to think of words or phrases between trials. This was marked with reference to Selection *D* (infra) but otherwise the tendency was negligible. In a few instances while listening to a selection when he was not supposed to recite, the subject did speak it softly to himself. On the occasions when there was an apparent tendency to depart from the regular program in the way of reciting between trials, no comment was made and the practice seldom persisted. With the exception of Selections *A* and *D*, which had served as controls in the two earlier series, the subject apparently had no notion as to which were original and which were control selections.

The main results together with some data from the earlier experiment by way of comparison are given in Table III. The first column gives the age at which the original reading took place. The Roman numerals in the next column are the arbitrary designations of the selections used in the eight-year experiment. The next column gives the number of repetitions necessary before the subject recited the selection verbatim without prompting. The data were analyzed from two standpoints, the number of repetitions necessary before each word in the selection had been anticipated and the number of repetitions required for reciting it verbatim without prompting. Previous analysis indicated, however, that there was little choice between these two methods of scoring and for the remainder of the discussion the data will be confined to the last method—the number of trials necessary for a complete verbatim recital without prompting. Such scores for the eight-year experiment occur in the third column.

TABLE III

TRIALS NECESSARY FOR CORRECT RECITATION

Age in Mos. at Original Reading	*Eight-year*		*Fourteen-year*		*Eighteen-year*	
	Selection	*Trials*	*Selection*	*Trials*	*Selection*	*Trials*
15–18	III	382	II	142	I	202
18–21	VI	253	V	139	IV	190
21–24	IX	385	VIII	169	VII	181
24–27	XII	379	XI	151	X	220
27–30	XV	328	XIV	145	XIII	160
30–33	XVIII	226	XVII	169	XVI	175
33–36	XXI	265	XX	127	XIX	193
Av. of all		317		149		189
Av. 3 early		340		150		191
Av. 4 later		299		148		187
Control	*A*	409	*D*	169	*G*	205
	B	451	*E*	151	*H*	193
	C	445	*F*	166	*J*	175
Av. control		435		162		191
					A	112
					D	37

The next two columns give similar data for the fourteen-year experiment and the last two columns for the third experiment. The lower portion of the table gives data for the control selections, that is, those which were learned de novo. These were arbitrarily designated by letters rather than by Roman numerals. In the last two columns Selections *A* and *D* appear a second time. They were included at the eighteen-year level to determine the difficulty of relearning a selection of this type which had been mastered 10 or 4 years previously.

The most general notion of the results may be obtained from the averages. It required 189 repetitions for a correct recital of the average selection in the present relearning experiment whereas for the corresponding control selections the average is 191. The difference obviously is negligible and of no statistical significance. By the other method of scoring (not shown in table) the corresponding averages are 172 and 175. The absence of any saving in the present case may be compared with the 27 per cent at eight years (30 per cent by the other method of scoring) and 8 per cent at fourteen years by either scoring method. Apparently the last four years were sufficient to eradicate completely any trace of the original stimulation in infancy.

As in the previous experiments, the selections were grouped and averaged so that the three which were presented earliest in infancy constituted one group and the four in later infancy a second group. The difference between these averages obviously is not significant in the present series.

Learning curves were plotted as in the previous series. The two curves for average relearning selection and average control selection are practically indistinguishable and not worth presenting here.

The gross number of repetitions necessary in the present study was much less than in the eight-year but slightly greater than in the fourteen-year experiment. The former difference is presumably due to greater maturity, better motivation, and an appreciation of the scientific importance of the experiment. The reason for the latter is less clear. It cannot be attributed to the mere lapse of time since the reading in infancy, because it characterized control as well as experimental selections. The subject reported occasional confusion of groups of syllables with similar groups which he remembered vaguely. It is quite possible that such similar groups were carried over from the fourteen-year experiment and caused this confusion. Indications of this carryover may be seen in the fact that Selection *D* which had been learned to the point of complete recitation at age fourteen was relearned at age eighteen in 37 repetitions, and on the nineteenth trial which was the first one on which he was allowed to attempt recitation the subject repeated 182 out of the 240 syllables. Incidentally on the second trial with that selection the subject stated that it sounded familiar and thereafter was suspicious that he had experienced it more recently than infancy. However quite early with Selection *G* (control) he stated that some words were vaguely familiar.

On the whole the experiment suggests that the effect of presenting nonsense material in infancy was very clearly manifest in relearning experiments at the age of eight and a half, traces were still apparent at the age of fourteen, but by the age of eighteen the effect had completely disappeared.

SUMMARY

Meaningless material (20-line selections of Greek drama) was read aloud to the subject daily beginning at the age of 15 months. Every three months a different set of similar selections was used as material and this procedure was continued till the age of three. When the subject was eight and a half years old he learned some of the original material by a modified prompting method in comparison with other material which he learned *de novo*. Similar experiments were conducted with still other material from the original selections and further controls at ages of fourteen and eighteen.

At the age of eight and a half the relearning required on the average 27 or 30 per cent (depending on the method of scoring) fewer repetitions than were needed to learn new (control) material. At the age of fourteen the corresponding saving was 8 per cent. At the age of eighteen there was no saving at all. At the age of eight and a half the original selections which had been presented later in infancy were relearned more readily than those presented earlier in infancy. No tendency of this sort was manifest in the two later series. Learning curves for experimental and for control selections were appreciably discrete in the first two series with the experimental curves rising more rapidly than their controls. In the eighteen-year series the curves were indistinguishable. The effect of the presentation in infancy was clearly manifest at the age of eight and a half, was still present but markedly less at the age of fourteen, and was completely gone at the age of eighteen.

V–LANGUAGE AND THOUGHT

One—The Expressive Nature of Early Vocalizations

M. M. LEWIS

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Is infant vocalization simply random behavior? Are sounds produced in an unpredictable and meaningless way? Lewis thinks not and presents a coherent theory of early speech.

In describing infant sounds, Lewis uses the symbols of the International Phonetic Alphabet. A description of this may be found in Webster's New International Dictionary. Many of the symbols used refer to sounds which are frequently designated by the same letters in English. However, some of the symbols of the International Phonetic Alphabet refer to sounds that do not occur in English.

The newborn child in a state of discomfort utters vowel-like cries, ranging from **e** to **ɑ**, and frequently nasalized. Soon the semi-consonant **ŭ** intervenes, followed by **h**, **l**, and **ŋ**. Finally the nasal front consonants **m** and **n** appear. Contrasting with these are the sounds he utters when comfortable. At first these are non-nasalized vowels, of indeterminate quality, are interspersed with the back consonants **ɡ**, **g**, **x**, **k**, and **r**. Then front consonants appear, mainly non-nasal, **p**, **b**, **t**, and **d**, together with the corresponding nasals **m** and **n**.

What light does this analysis throw upon the expressive character of the child's early sounds? The answer lies in considering their phonetic nature, that is, in noticing which movements of the vocal organs are required to make them.

PHONETIC FEATURES OF THE CHILD'S EARLIEST SOUNDS

We may first of all be allowed to recall that phonetically there is no clear line of division between vowels and consonants. If in the course of utterance we contract some part of the vocal apparatus so that complete or partial closure results, the sound produced has the quality of a consonant; in the absence of such closure the sound has the quality of a vowel. Evidently there will be instances of sounds which will seem partly vocalic, partly consonantal, in character. For example, if when uttering the the vowel **u** we contract the lips still further, the sound takes on the quality of a consonant (ŭ); in this way we may speak of the child's ŭ and **ŭe** as semivocalic.

The child's earliest sounds, as we have seen, are vocalic; consonants begin to appear in his utterance when the contractions at different points of the vocal apparatus become more definite. We may regard this as one instance of that increase in definiteness which marks all the child's motor activities. During a period, therefore, which extends over several months, it is not always easy to decide whether a given sound is vocalic or consonantal.

With this reservation, we may examine the phonetic characteristics of the child's utterance, and consider the relation between the specific sounds and the effective states which they express.

THE EARLIER DISCOMFORT CRIES

We begin with the principle that there is an innate tendency for a child to vocalize when in discomfort. If he were merely to open his mouth widely and phonate, the sound would be heard as **ɑ** and this does occasionally occur. We might call this the primary open vowel, for when we produce this sound **ɑ,** the tongue is in a more or less relaxed position, resting on the floor of the mouth, and the lips are also in a medium position between the extreme rounding which occurs in **u** and the extreme stretching which occurs in **i.**

But many of the earliest cries of the child are not simply produced by opening the mouth as widely as possible. He is in discomfort, and as Darwin has pointed out (6) the mouth of a child in such circumstances tends to take on "an oblong, almost squarish outline," the upper lip being raised by the contraction of the muscles round the eye—a reflex mechanism, as he thought, to protect the eyeball from becoming engorged by the increase of blood pressure in the head and face. Darwin himself was not inclined to lay much stress on this explanation, but of the accuracy of the observation there can be no doubt—clear instances are given in the photographs of crying children in his book.

Let us notice what happens to the open sound ɑ when the mouth is drawn into this oblong shape; it inevitably changes its quality and becomes a—precisely the sound which is most characteristic of the child's earliest cries. This possibility, that the shape of the mouth—itself due to expressive muscular contractions—might give rise to expressive differences of speech, occurred to Darwin, although he did not attempt to apply it to the cry of the child. Following up the clue given by him, we can say that the characteristic form of the child's cry a may certainly be called expressive, for it is the direct vocal manifestation of the contractions which occur in a state of discomfort.

We are not saying here that the muscular contractions are the "cause" and the sound a the "effect." What we are pointing out rather is this: that specific contractions are the innate accompaniment of a state of discomfort, that vocalization is another accompaniment of this state, and that when vocalization and the contractions occur simultaneously, we hear the sound a.

With this clue before us, let us now look at the other sounds which we have found to be characteristic of the child's earliest cries. First we have the further narrowing of the primary open sound ɑ in the direction of ε and e. Now it is interesting to notice that these sounds may be produced if the *pitch* of the voice is raised while uttering a.* And, as we are again reminded by Darwin, shrillness is an expressive characteristic of the cries of animals when in pain. Thus e and ε may also be regarded as expressive in nature: They are the vocalic modifications which result from the expressive raising of pitch during a state of discomfort.

A further marked characteristic of utterance at this stage is nasalization. This too we may show to have an expressive basis. For nasalization in speech occurs very frequently when we wish to produce a greater volume of sound; it is well known that some public speakers tend unconsciously to nasalize their speech in the effort to increase its carrying power. The volume of sound is certainly increased by nasalization: Paget, working with his artificial resonators, found that the addition of a nasal cavity (the equivalent of nasalization in human speech) invariably had this effect.

We may reasonably suppose that nasalization occurs in the child's utterance, as he tries to scream his loudest under the stress of hunger or other discomfort. If this is so, nasalization may well be regarded as expressive in the Darwinian sense, in that it is a quality taken on by the child's cries as he tries—unconsciously—to increase their volume. This is a point of some importance when we come to consider the nasal consonants which appear later.

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* This has been demonstrated experimentally by Englehardt and Gehrcke (7) by means of gramophone records played at varying speeds.

We pass now to the group of sounds **ŭ**, **l**, **ŋ**, **h** which commonly occur interspersed among the child's early vocalic cries: **ŭ** and **h** from the outset, the others later. Phonetically, these four sounds may all be characterized in this way: They occur during the utterance of a vowel if the vocal organs are momentarily either allowed to relax or to contract. The child does not utter one prolonged vowel; he produces a series of rhythmical cries: for instance, ŭe-ŭe-ŭe . . . or le-le-le . . . , the one vowel **e** in fact being broken up into a series of cries, as the mouth partially contracts for a moment before letting out the next bellow. The contraction however is not usually sharp enough to produce the effect of a definite consonant.

Now there are, roughly speaking, four points in the vocal apparatus at which a vocalic cry might be modified: the lips, the front of the tongue against the hard palate, the back of the tongue against the soft palate, and the vocal chords. The first of these modifications would give the quality of ŭ, the second that of l, the third that of **g**, or with nasalization **ŋ**, the fourth that of **h**. For if the lips partially contract while a vowel is being uttered, the semiconsonantal ŭ will tend to appear; the vowel **a** or **e** or whatever it may be, takes on for the moment the character of ŭ. Again, if the tip or blade of the tongue happens to touch the hard palate—while the sides of the tongue are still lowered to allow the breath to pass out—l results. This may happen during the utterance of any vowel, the main part of the tongue being still in position for that vowel. Again, during the utterance of the nasalized vowels which tend to appear in crying, the back of the tongue is raised and the uvula correspondingly relaxed so that the breath may pass out through the nasal passage. If now the back of the tongue is contracted further toward the soft palate, **ŋ** results. Finally, if the vocal chords, which are being held close together during the production of a vowel, are relaxed for a moment, **h** appears; to cite Jones (11), "**h** is the fricative sound heard as the air passes through the open glottis, the other organs being in position for the following vowel." In other words, **h** is the sound heard if the voice is momentarily withheld in the act of uttering any vowel.

Each of these four semiconsonants is therefore expressive. The child's vocal organs function rhythmically; as Wundt points out (15), we may regard this as an instance of the rhythm which characterizes the utterance of all animals. The child, in fact, utters not one prolonged cry, but a series of cries, and as the vocal apparatus is contracted or relaxed at the beginning of each successive cry, the listener hears a modified sound which is consonantal in quality. Then, according to the place at which the modification takes place, it is heard as something approaching **ŭ, l, ŋ,** or **h.**

Thus we are able to show that all the chief characteristics of the child's discomfort cries—vocalic and consonantal—are expressive, in the accepted Darwinian sense of this term.

THE EARLIER COMFORT SOUNDS

Let us pass to the comfort sounds. Here again we begin with the principle that there is an innate tendency for the child to vocalize when in a state of comfort, although this vocalization makes its appearance only when the expression of discomfort is already well established. But why should the sounds uttered in comfort take the form of the back consonants, **g̡, x, g, k,** and "guttural" **r**?

The first point to notice is that all these five consonants are produced at the same spot in the mouth—by raising the back of the tongue so that it comes into contact with, or approaches, the soft palate (velum). The difference between the pair of spirants, **g̡** and **x,** and the corresponding stops **g, k** is merely that the former pair is uttered during narrowing by a continuous emission of breath, the latter pair by a full closure followed by an explosion of breath. Further, in the first pair, **x** differs from **g̡** only in being unvoiced, that is, there is little or no vibration of the vocal chords in its production; in the second pair, **k** differs from **g** precisely in the same way. And "guttural" **r** is produced at the same point of the mouth, or slightly farther back, by a rapid series of broken contacts between the back of the tongue and the termination of the soft palate—the uvula. In the case of the child, this **r** may be regarded as a rolled form of the sound **g̡.**

This voiced spirant **g̡** is the most characteristic sound of the group. It is a sound which—as made by the child—does not happen to occur in any European language, with the result that different observers have resorted to different signs in recording its occurrence in children's cries. It is the sound indicated by Stern (14) as *rr* in *erre,* for he tells us of this, that it resembles both guttural *r* and *ch* as in *doch*—a good description of the spirant **g̡.** This is the sound, he adds, that is the earliest comfort sound to be noted by the majority of observers—the sound recorded by Preyer (13) in *örrö, arra.* To this we may add further that from Hoyer's account (9) it must be the sound recorded by him as **g̈.**

Why should the raising of the back of the tongue toward the soft palate occur in a state of comfort? One answer—that of Grégoire (8)—is that when the child is lying comfortably on his back, his tongue naturally lolls against his soft palate, producing the characteristic sounds. But we can perhaps suggest a more exact answer. Under what conditions do these sounds appear? On this point, observers are fully agreed. The normal child during his first weeks, manifests pleasure, if at all, mainly after feeding: when he is lying satisfied in his mother's arms. It is then that "smiles" and gurgling noises appear, not very easily distinguishable at first from belchings, gruntings and signs of "wind." In the case of K, observed by me, the first certain instance of ɡ occurred when he was just six weeks old, lying on his back very comfortable after a feed. A few faint utterances of this sound appeared interspersed among grunting noises, accompanied by those face and lip movements which are regarded as signs of "wind."

This is a possible clue to the origin of back consonants in states of comfort. The movements which take place in the back of the child's mouth as he expels "wind" are, if accompanied by phonation, heard by us as guttural grunting, some of which approximate ɡ.

There is, however, a further condition which determines the child's utterance in a state of comfort after a feed. Some swallowing movements are likely to occur, partly because of the excess of saliva which will have remained in the mouth, partly perhaps as the persistence of reflex movements of those organs which have been working rhythmically throughout the period of feeding. Now the movement of swallowing involves just that approximation of the back of the tongue to the soft palate which, if made during phonation, would be heard as sounds of the ɡ-type.

It is clear then that the production of this back consonant ɡ probably arises directly out of the movements intimately connected with the business of feeding in which the child has just been engaged. If we are willing to accept the principle that there is an innate tendency in the child to utter a cry expressive of his state of satisfaction, then the facts we have mentioned help to show why the cry should take on the form of this specific back consonant ɡ. It is the vocal manifestation of the fact that the child is making guttural movements expressive of satiety while uttering a cry of satisfaction. As for the sounds x, k, g, and uvular r, these are all either voiced or unvoiced variations of ɡ, and have the same expressive origin; they arise as movements inherently connected with the state of satiety which they manifest.

Do these back consonants express other states of comfort in addition to satiety? On this point the published records are not quite clear, the observer usually indicating no more than the fact that the child was making the sounds in a state of comfort. In my own observations I certainly noticed that throughout the child's earlier months, whenever he used back consonants with the characteristic intonation expressing comfort, it was immediately after a feed. But Preyer, Stern, and Hoyer all give cases of the occurrence of these consonants as expressive of contentment without mentioning definitely that this was a state of satiety: and a curious instance of the wider function of the expressive back consonants occurs in an early account of the blind deaf-mute Laura Bridgman given by Lieber in 1851 (12). "Frequently I have heard Laura expressing a feeling of satisfaction by a subdued tone, somewhat between chuckling and a slight groaning." He adds quaintly, "I would have said *grunting*, as more accurately expressing the sound, had I not felt reluctant to use this word in connexion with that amiable and delicate being."

It is clear that a transition of this kind might readily occur, so that sounds expressive of satiety would ultimately become expressive of other states of satisfaction. In the child's earliest weeks, as Charlotte Bühler points out (2), the *only* time when he expresses satisfaction at all is when he is satiated, immediately after feeding; when he is not asleep, he is either crying, taking food, or resting peacefully after feeding: Only in this last state does he express satisfaction. As time goes on, the periods of waking satiety grow longer: The child begins to notice—with apparent pleasure—the things about him, and then to play. And because these periods of contentment are extensions of his states of satiety, the guttural sounds which expressed his satisfaction after feeding will tend to become expressive of his comfort in general.

THE FRONT CONSONANTS

We came finally to those consonants which are so important in relation to the first "words" uttered by the child: the nasals **m**, **n**, the dentals **t**, **d**, and the labials **p**, **b**. What are the physiological conditions of their formation; are they to be regarded as expressive?

The first point to notice is that **m** falls into a group with the labials **p**, **b**; **n** into a group with the dentals **t**, **d**. The three consonants of each group are formed by contact at the same part of the mouth. In the labial group, **m**, **p**, **b**, the lips are brought together; but for **m** the uvula is lowered towards the back of the tongue so that the breath passes out through the nose in a continuous stream, the difference between **p** and **b** being only that the former is unvoiced. In the same way, in the dental group, all three consonants **n, t, d** are formed by contact of the fore part of the tongue against the upper gum ridge. For **n** again, the lowering of the uvula allows the nasal emission of breath, while the difference between **t** and **d** is once more only of voice. Thus we have a labial group **m, p, b,** and a dental group **n, t, d**; the nasal member of each group being characteristic of a state of discomfort.

The connection between these labial and linguo-dental movements and those involved in sucking is obvious at once. It has often been mentioned in a general way, as for instance by Wundt (15) and Jesperson (10). But no attempt has, I think, hitherto been made to analyze the phonetic basis of this similarity.

If we observe the movements made by the lips and tongue in sucking, we find that it is essential for the lips to be brought together, while simultaneously, or immediately afterwards, the front of the tongue is pressed against the upper gum ridge. The former of these movements is precisely that involved in saying **m, p,** or **b**; the latter in saying **n, t,** or **d.** How then do these consonants become expressive of discomfort or comfort?

FRONT CONSONANTS EXPRESSIVE OF DISCOMFORT

A common observation helps us here; at a very early age the child is found to be making anticipatory sucking movements when he is hungry or expects to be fed. Darwin (5) noticed it as long ago as 1877 in the case of his own child, thirty-two days old; and the fact has been repeatedly corroborated by careful observers such as Bühler and Hetzer (3) who noticed it of children in their second month. Curti also (4) reports some Russian observations which go to show that if the child, in the third or fourth week, is merely held in the normal feeding position, this will be sufficient to evoke sucking movements, in the absence of any other stimulus.

In the case of K, these sucking movements were very marked. From his first days he was accustomed to having a bib tied under his chin before being given his food. After his thirty-fifth day, when the bib was tied at feeding time he definitely pursed his lips into the sucking position, before he was brought within reach of the food. Nine days later, at the time of his evening feed, this reaction was most definite. "He was crying lustily. His mother put the bib under his chin. His cries gradually lessened, and ceased within about 30 seconds. Then he very definitely began to make sucking movements. His lips were pursed, and the tip of his tongue slightly protruded."

We are here introducing a third factor into our discussion—that of training or conditioning—in addition to our two principles of Darwinian expression and the differentiation of affective states. But it is a factor that is certainly not in dispute. We may take it as a generally accepted principle that when an organism has experienced a pattern of events, the recurrence of any salient item of this pattern will tend to cause the organism to behave as though the rest of the pattern were present. Here the child when hungry has felt the bib being tied under his chin, and this followed by feeding; sooner or later the touch of the bib when he is hungry, or the mere sight of preparations for food, will evoke sucking movements appropriate to feeding.

It is evident that if the child phonates while making these anticipatory sucking movements, the sounds produced will approximate to the labial and dental consonants. If, further, he is in a state of discomfort such as hunger, and phonates *nasally*—as we have seen he does—then the consonants produced will inevitably be **m** and **n**. Many observers have noticed that these nasal consonants appear when the child is hungry, but I do not think that they have been shown to arise out of anticipatory sucking movements made during nasal phonation.

The occurrence of these front consonants is so marked—they are indeed the first definite sounds that a casual observer of a child notices—that their origin has sometimes been ascribed to imitation. Wundt, for instance reminds us that mothers commonly make a sound like *mum-mum* when coaxing their infants to take food, and obviously this might have some influence upon the child's own utterance. But it is clear that imitation can at most be only a secondary factor in the production of these front consonants. For neither blindness nor deafness in children seems to prevent their occurrence. Wundt himself cites the case of a deaf-mute aged nineteen who habitually used *mum* to signify food. Jespersen doubting the influence of imitation on the production of the front consonants, suggests that we should find that they regularly occur in blind children also. Since the publication of his work a case has been described by Bean (1); the child, who was afflicted with congenital cataract in both eyes, nevertheless uttered *ŭa, ŭa* in states of discomfort soon after birth, and *mmuh* at the age of 0-3,2, when hungry.

There can be little doubt that the origin of the nasals, **m**, **n**, is that they are expressive of hunger; that is, they are the audible manifestation of the mouth movements which are bound up with that state. And because the expression of hunger is the most important function of the child's utterance during his earliest months, these nasal sounds become one of the chief means by which the child enters into linguistic communication with those about him.

FRONT CONSONANTS EXPRESSIVE OF COMFORT

The appearance of front consonants—**p**, **b**, **t**, **d**, sometimes **m** and **n**—in a state of comfort is not so easy to observe as the consonants expressive of discomfort. The child when contented does not articulate with the same definiteness as when moved by the urgency of discomfort. Yet if one listens carefully, one hears sooner or later, emerging from the welter of semiarticulate guttural sounds, consonants which approximate to the labials and dentals.

They occur when the child is contented; chiefly, that is, either after a feed or when he delightedly perceives his food already before him. If we bear in mind this fact of their appearance, their expressive nature is at once evident. They are the vocal manifestation of sucking movements, made usually without nasal urgency, the child uttering them while phonating contentedly on seeing food, or while continuing to make sucking movements after a feed. And, just in the same way as the back consonants are expressive of satiety, these front consonants may ultimately become expressive of other states of contentment.

One tendency will clearly bring all the child's early consonants into the stream of contented utterance—the rise of babbling or vocal play. And an important feature of this activity, we shall show, is that even consonants which are primarily expressive of discomfort may become transformed into play, and so appear in a state of contentment.

RESULTS

Our phonetic analysis of the child's earliest utterance has enabled us to demonstrate its expressive character, in the Darwinian sense. In particular, we have found that the origin of the earlier back consonants and of the latter front consonants lies almost entirely in the expression of hunger and of its satisfaction. This is not surprising, for by far the greatest part of the child's life in his earliest months is taken up by food and sleep. And it is inevitable that any utterance made by the child in connection with feeding should become shaped by the movements of feeding; in discussing the nature of language we cannot evade the fact that the organs of utterance are also the organs of sucking.

We have shown also that some order and regularity may be found in what has often been regarded as the chance and chaotic occurrence of the child's earliest sounds. We have shown that they fall into groups which may be explained by referring to three generally accepted factors—the principle of Darwinian expression, the differentiation of affective states, and the occurrence of behavior anticipatory of a sequence of events.

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Two—The Conditioned Response as a Basis for Language

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Lewis has indicated how early sounds are made and under what inner conditions they appear. Allport extends the theory of infant speech by showing how the early sounds become associated with sounds produced by adults and how they become associated with objects.

Many earlier writers presented essentially the same theory, but Allport's statement is one of the clearest and fullest.

THE DEVELOPMENT OF LANGUAGE

Stage 1. Random Articulation with Fixation of Circular Responses. The marvelously intricate and versatile speech mechanism is at birth, like other motor mechanisms, simply a crude possibility. Further growth of the nerves and muscles must combine with *practice* to produce a repertory of sounds adequate for language. With such development as a basis, the social environment furnishes the stimuli necessary for the acquisition of perfected speech habits. The earliest used consonants, which, according to Miss Blanton, occur during the first month of life, are chiefly nasals and gutturals, such as **m, n, ng, g,** and **k** (also **h, w,** and **y**). These represent easy mouth positions adopted probably as random movements. They are articulated with various long and broad vowel sounds, and with some diphthongs (double vowels), as in "gow" (writer's son at two months).

Overlapping with the period of laryngeal expression and finally succeeding it there appears the stage of random articulation, the babbling and cooing of the child during its second and third half years of life. In this period the early consonants are repeated with better control and supplemented by new ones. The dental and labial explosives, **p, b, t,** and **d,** are soon acquired. The fricatives **s, f, v,** and **th** are more intricate and come later. **L,** which requires inversion of the tip of the tongue, may require three years to perfect. **R** also is difficult as an initial sound. Examples of early mispronunciations are "whing" (sing), "yight" (light), and the like. Double consonants cause much difficulty, the second consonant generally being slighted; for example, "p'ease" (please).

With random articulation we enter upon a new phase contrasting somewhat with that of pure laryngeal utterance. The latter is imperative in mode. It arises with strong, unpleasant emotion due to thwarting or discomfort; and it rapidly assumes the function of social control. "Baby talk" on the other hand is spontaneous and indicative of a pleasant mood. It is a form of play, a part of the diffuse outflow of energy, rather than an effort at the control of others. If stronger emotions enter the field, bringing in the functions of the sympathetic nervous system, the pleasant prattle at once gives place to the inarticulate cries of the earlier period.

Too much attention has been paid to the acquisition of vocabularies, and too little to the study of the preverbal stage of random articulation in infants. This stage not only affords the material for language but gives the practice necessary for the control through the ear of the muscles of speech. The chief significance of the vocal play of babies seems to be in establishing circular reflexes between the sound of the syllable and the response of speaking it. Let us suppose, for example, that the baby utters the syllable *da*. By so doing he stimulates himself through two channels. He receives certain kinesthetic sensations from the sound which he produced. It is with the auditory stimulation that we shall be chiefly concerned. Returning to the brain centers these afferent impulses are, or tend to be, redischarged through the same motor pathways as those used in speaking the syllable itself. There are two possible methods of explaining this. We may suppose that the synapses connecting the afferent impulse with the motor outlet of speaking *da*, having been recently used, are in a state of relatively lowered resistance, and are therefore readily put into operation again. Or we may infer that, in some cases at least, the return stimulations are received while the speaking response is still going on (as in a prolonged vowel sound), and the motor synaptic resistances for *da* are completely overcome because discharge through those synapses is actually taking place. We have here the exact situation for the formation of a *conditioned response*. The response *da* becomes circularly conditioned by the sound *da;* and this sound when later heard will tend of itself to evoke the response of speaking it. This latter explanation is probably the true one. While the baby is practicing the syllabic elements of his future vocabulary, he is therefore also fixating ear-vocal reflexes through which a spoken sound may directly evoke its enunciation. Articulation has now advanced to a stage where it is capable of being controlled through the auditory receptor. The process just described is illustrated diagrammatically in Figure 1, A.

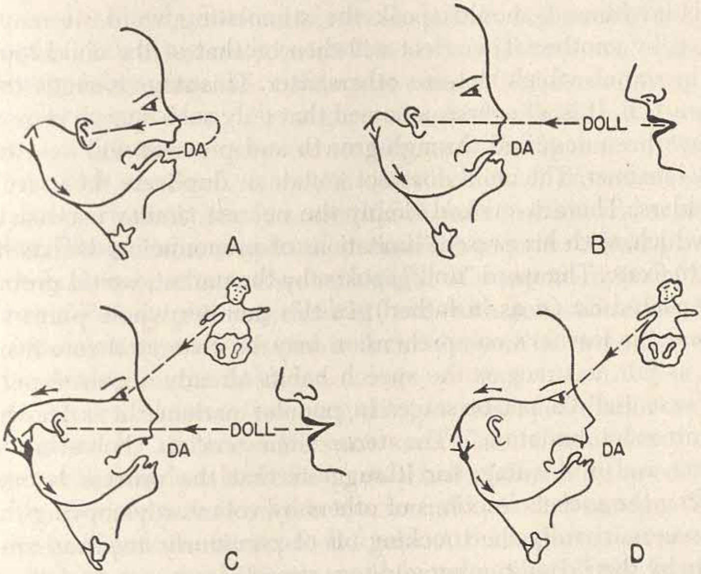

Figure 1. The development of language habits in the infant.

A (Stage 1). Random articulation of syllables with fixation of circular responses. Chance articulation of the syllable da *causes the baby to hear himself say it. The auditory impulse is conveyed to the brain centers, where it discharges into the efferent neurons to muscle groups used in pronouncing the same syllable. An ear-vocal habit for* da *is thus established.*

B (Stage 2). Evoking of the same articulate elements by the speech sounds of others. An adult speaking the word "doll," which is closely similar to da, *causes the auditory excitation again to discharge into the response* da.

C and D (Stage 3). Conditioning of the articulate elements (evoked by others) by objects. In C the process shown in B is repeated. A doll shown at the same time stimulates the baby's eye and forms a visual connection with the motor neurons being used in pronouncing the syllable. There is thus established a conditioned response between the sight of the doll and the speaking of da. *The sight of the doll alone (D) is now sufficient to evoke its name (*da *being as close as the baby can come to the pronunciation of "doll").*

Stage 2. Evoking of the Articulate Elements by the Speech of Others (so-called "Imitation"). At this point the social influence enters the process of language development. If the ear-vocal reflexes have been sufficiently established for the sound of a word to call forth the response of articulating it, it is no longer necessary that the child himself should speak the stimulating word. It may be spoken by another. The effect will then be that of the child repeating the sounds which he hears others utter. This stage is suggested in Figure 1, B. It is, of course, assumed that only such speech responses as have been acquired through growth and practice will be evoked in this manner. The child does not imitate or duplicate the speech of his elders. There is evoked simply the nearest similar ear-vocal reflex which, with his present limitations of pronouncing, he has been able to fixate. The word "doll," spoken by the parent, would probably be repeated *da* (*a* as in father). In this manner whole phrases far beyond the learner's comprehension may be reiterated rote fashion with as fair accuracy as the speech habits already acquired permit. It is essentially a parrot stage. In popular parlance it is known as "learning by imitation." The term "imitation" is, however, both inexact and misleading, for it suggests that the process is one of learning the speech reactions of others by voluntarily copying them; whereas it is really the touching off of *previously acquired* speech habits by their conditioning auditory stimuli.

Discussion of the Theory Involved in Stages 1 and 2. The reader should bear in mind that the process thus far described is largely hypothetical. Precise physiological data are wanting; but in their absence we may review certain lines of evidence in support of the hypothesis.

1. If vocal responses are circularly fixated, with the sound of speaking them serving as stimulus, we should expect that reiteration of the same syllable over and over would be a necessary result. The baby would learn to mimic himself as a prerequisite for repeating sounds made by others. The facts support this supposition. Reduplication of syllables (*da-da-da*) in a tireless manner is a common phenomenon of baby talk. Later many objects are named with doubling of syllables (for example, "wah-wah" for water), and longer phrases are reiterated as a kind of play.

2. Only sounds which have been already pronounced in random articulation can be evoked by the speech sounds of others. That is, only those sounds can be evoked which have had a chance to become circularly fixated as ear-vocal reflexes. The spoken word "pencil" was repeated by the writer's son as *punka* (c and l sounds

not yet acquired). The phrase "What is that?" involving difficult consonants, was reproduced as ŭh ĭ ă. The words "down," "doll," and "clock," when spoken to him, were all repeated as *da*. *Ba*, similarly, was his reproduction of "box," "bath," "bottle," "block," and "bye."

3. There exist in the central nervous system mechanisms adequate for the circular fixation of vocal habits. Leaving out of account the cortex, relatively undeveloped in infancy, there are adequate connections between the auditory nuclei of the brain stem and motor fibers controlling the organs of speech. Neither high intelligence nor conscious imitation are necessary for the use of this apparatus. The ear-vocal connection is direct and immediate. The evidence for this is at hand in cases of *echolalia* in idiots and asphasic patients. These "human parrots" accurately reiterate whole phrases spoken in their hearing without the slightest comprehension of their meaning. We are probably dealing here with subcortical mechanisms representing early formed and circularly fixed responses comparable to those of the baby.

4. It is well known that congenital or early deafness is usually accompanied by mutism. Deaf-mutes are able to articulate in the manner of the random infantile period (baby talk); but they cannot, without special methods, learn the use of spoken language. Since the ear-vocal reflexes were not and cannot be acquired, some other form, such as eye-vocal reflexes, must be substituted if the knack of speaking words is to be imparted to them. The lack of the usual, early formed, circular vocal reactions is responsible for their mutism.

Without pursuing this question further we may tentatively accept the foregoing explanation of the so-called "imitative stage" of language development. Word habits have been formed which are capable of being put into effect by the sound of the same words spoken within hearing. The next step is to convert these parrot-spoken reactions into true language. This step, like the preceding, is achieved through social agencies.

Stage 3. Conditioning of the Articulate Elements (*evoked by others*) *by Objects and Situations.* As soon as the stage is reached in which the parents can evoke repetitions of words from the infant at will, the process of teaching him to name objects begins. It does not suffice to say "doll" and hear the child repeat *da*. The doll itself is held up for inspection while the learner repeats the word pronounced by the parent or nurse. A conditioned response is thus formed; the afferent visual impulse from the doll discharges its energy through the motor pathways of the speech pattern of pronouncing the word. The object itself thus becomes a stimulus adequate for evoking the response of speaking its name. Figures 1 C and 1 D, illustrate schematically this conditioning process. Stages two and three are practically synchronous in the actual development of the child. We have separated them in the description only for the sake of clearness.

Progress from this point is rapid. A child may learn in this manner to speak the approximate names of several hundred objects while he is still laboring over the exact pronunciation of difficult consonants. The naming, or vocabulary-acquiring, process begins early in the second year and increases by ever-lengthening strides up to six years, at which age the average child has a vocabulary of about 3,000 words.

Our explanation thus far has involved only the control of the speech reactions of the child by the adult. Social control, however, soon operates in the reverse direction. The child learns to use his naming habits as *demanding* habits. Suppose he sees a new and interesting doll out of reach on a shelf. Manipulative tendencies cause him to reach for it. Failing in this, the usual law of trial and error brings into play all possible movements. One of the readiest and easiest of these movements is the pronunciation of the word "doll"—a reaction which is moreover elicited by its recent association (conditioning) with the sight of an object of that general sort. The word is therefore spoken, and the pleased parent presents the doll as a reward. The manipulative drive now proceeds unhampered, and the arcs involved in this solution of the problem are fixated for future use. By simple vocal expression the child thus learns to control others. He increases vicariously his own stature, his power, and his sagacity by enlisting these attributes of adults in the service of his needs. Little wonder that his linguistic progress is rapid!

The naming reaction can be conditioned not only by the sight of an object but by other stimuli inherent in the *general situation.* The word "doll" may have been evoked at a time when the child was handling the toy, "talking" to it, or even running to get it. The proprioceptive stimulations arising from these acts therefore become adequate conditioning stimuli for producing the response of speaking the word. In all relations in which the doll itself was formerly experienced, the word "doll" may now be called up in consciousness and evoked as an audible or a "thought" response. At any future time therefore when the child may recall or have the tendency to manipulate such an object through habit, he will be likely to say "doll." The attendant again produces the object; and the arcs involved in this solution are fixated as before. The learner has now reached the advanced stage of demanding objects *desired but not seen.* Verbs, adverbs, and particles, such as "give," "down," "again," "move," and "no," are acquired and used in the same fashion. Having been learned through social agents in connection with attitudes, postures, and situations, they are now used to control these agents with respect to the situations they represent.

In the learning of language then, as in the stages of laryngeal and gestural expression, we find that social control is a cogent factor. With increasing development, however, other considerations enter. In addition to naming and demanding objects the child begins to talk *about* them. He discourses to his toys and about them. He verbally reviews bits of the day's experience as he lies in his crib in the evening, and in so doing substitutes word responses for the overt movements he originally employed in living them. In other words, language becomes for him a vehicle of *thought*.

DEVELOPMENT OF RESPONSE TO LANGUAGE

A few words may be added concerning the understanding of language by the infant, a function which precedes its actual use by some weeks or months. Speech sounds of others stimulate the child in many ways beside the eliciting of ear-vocal reflexes. They control his behavior in consoling him, diverting his attention, and offering signs by which he knows that he is to be tended in various ways. Language serves to condition the activities of the baby in the same way that the incidental growls or sex sounds condition the withdrawing or approaching responses of the lower animals. Experiments show that dogs respond very little to words as articulated symbols, but chiefly to the pitch, intensity, and quality of the voice. The earliest effect of vocal stimuli upon the baby is through these same laryngeal components. An infant will cry at a scolding tone of the parent long before the words themselves are understood. By the end of the first year the response to commands, or to the direction of attention, that is, to some part of the child's body, indicate that he is beginning to understand the meaning of articulate word symbols.

The final achievement of linguistic development is the response to language by the use of language, as in answering a question. This occurs late, usually after a fair mastery of speech has been obtained. Aside from the intellectual difficulty involved, there appears to be a kind of inertia: The child is loath to quit the placid, irresponsible haven of ear-vocal reflexes for the uncharted sea of interrogation.

Three—The Expansion of Meaning

M. M. LEWIS

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Once a sound is used in a specific context, how is it extended to other situations? This is the problem that Lewis discusses in the following section.

We have used only two short selections from Lewis' book INFANT SPEECH. *The student interested in language will find the entire book very stimulating and informative. A very extensive survey of the writings on language development in children is McCarthy's chapter in Leonard Carmichael,* MANUAL OF CHILD PSYCHOLOGY.

A marked characteristic of children's early use of words is that both in response and in utterance they will apply a word to a wide range of situations extending far beyond those in which it was first acquired. K, for instance, came to respond to "Where's ballie?" (0-10+27) by holding up a large colored ball in place of the original small one; and—in the development of an uttered word—he grew to say *tittit* (1-8+28) on seeing the church clock, having previously used this word for his father's watch.

Such extensions are so striking and obviously so important that they have never failed to obtain a good deal of attention. Perhaps the best known case is that reported by Romanes of a child in his first year who having learnt the word *quack* for a duck, applied it to the figure of an eagle seen on a coin, and then to coins in general. The Chamberlains (3) tell us of their daughter that having at 1-7 learnt the word *mooi* with reference to the moon, she proceeded to apply it to cakes, round marks on the window, writing on the window, writing on paper, "round things in books" tooling on books, faces, postmarks, and the letter O. Lindner (7) records that his wife had made a sledge (*schlitten*) out of a postcard for the daughter at 2-0. A month later, when the toy had long been destroyed, the child said *mama litten* on seeing a postcard delivered at the house, and some months after this again *litten* on seeing a letter. Numerous other instances of this kind of extension occur in the records of a host of observers.

The records of most of these observers consist of a list of the various meanings which have been given by a child to one and the same word. Observations of this kind, valuable as they may be as illustrations, do not throw much light on the actual process of extension. For this we need a fuller statement; we need to know in some detail how the child behaved in using or responding to the word on successive occasions. Records of this kind are rarer; the only ones in fact at all full enough for this purpose are those published by Ament (1) in 1899 and Idelberger (6) in 1903. To these I venture to add some of my own observations of K.

EXTENSION AS THE GROWTH OF GENERALIZATION

How are we to explain this wide use of words? The most obvious view is that the child has begun abstraction and generalization; abstracting from diverse situations some features which they have in common, and thus grouping all these situations into the same class. Most of the things, for instance, named *mooi* by the Chamberlains' (3) little daughter were circular in shape. The child extends the use of the word from the situation in which it was first acquired—seeing the moon—to other situations more or less like it.

There is obviously some truth in the view that here we have a clear step in the development of conceptual thinking—and this is a point which has been fully recognized, from the time of the early observers—Sigismund, Taine, Darwin, and Romanes—right up to our own day. But no one today would, I think, be satisfied with this as a full explanation. At least three other factors must be taken into consideration. First we must recognize that this broad use of words does not begin at the moment when the child acquires conventional speech: it is a gradual development of his earlier linguistic activity. Secondly, it is not enough to speak of the objective similarity of situations for the child, neglecting the affective responses they arouse in him and their functions in his behavior. Thirdly, we must not forget that the word itself is an instrument for the child, a tool, and that this certainly determines the manner in which it is used.

In these three ways we have to supplement the account given even by Stern (10). For although he refers to other factors, on the whole he confines himself to a summary of those kinds of *objective* similarity in diverse situations which will cause a child to extend a word from one to the rest.

THE EARLY WIDE USE OF SOUNDS

To take full account of the process it will not do to begin at the point where the child first acquires conventional words from the adult language. We must go further back into his linguistic history. If, for instance, we find Stern's daughter Hilde in her twelfth month applying *puppe*—used first for her doll—to any one of a large number of her toys, we have to consider the possible connection between this behavior and the sounds which she made while playing with these toys before she learned their conventional names. For our study of children's language leads us to assert that its development is continuous, not—as some writers such as Bühler have maintained—a series of sudden steps. The history even of the child's first conventional words does not begin at the moment when he acquires them; they have their root in his earlier use of sounds. In a real sense we can say that no word used by the child can be totally new to him; every word has affinities with his earlier linguistic experience.

With a record of observations such as those of K, it is not difficult to trace the changes in the child's use of sounds, preceding his adoption of conventional words. We find that even at the outset the child's sounds are used in a wide rather than a limited way. This is true both of his utterance of language and of his response to it. As for his response to language, we found that when he hears a particular pattern of intonation and sound he will respond at first not always with the same movement but with any of a number of movements—all expressive of the same affective state. And in the development of his utterance, we find that a particular sound such as **ɑ** is called forth at first not by a particular situation, but by any one of a number—again alike in this, that they arouse the same affective state.

In tracing the development of such a sound we were able to observe these stages: First its use with varied intonations to express different affective states; second; the increase of reference to a situation in the use of the same sound; third; the adoption of a conventional substitute for the sound, such as **fɑ** (flower), or **pei** (airplane), this bringing with it a further increase of objective reference.

The next stage of development is that which particularly interests us here: The child extends the use of the conventional word to situations other than those in which it has been acquired. Thus K, having at 1-6+14 begun to use the word **fɑ** on seeing some hyacinths, used the same word on the following day for tulips, six days after this for irises, and again six days later (1-6+27) on being wheeled beneath a flowering cherry. Four months later, at 1-10+26, he said the same word on seeing a biscuit decorated with a flower in sugar, and again on seeing embroidered flowers on a pair of slippers.

A similar development took place in the case of the word **pei,** which the child used for the first time at 1-9+27 on seeing an airplane, in place of his customary **e e**. Next day he repeated the word on seeing not an airplane, but a child's kite.

Now the important point that emerges from these series of records is that it clearly is not possible to draw a hard-and-fast line at any particular moment in the child's development and say: Here the extended use of sounds begins. For while the child is still in the stage of uttering his own primitive sounds he already uses them in an extended fashion. K, for instance, used **ɑ ɑ** at 1-4+8 on smelling a handkerchief and again at 1-4+12 on smelling jonquils. Similarly, he used **e e** at 1-1+26 while reaching out for milk, at 1-4+1 for cake, and up to 1-8+16 for butter; again he used the same sound at 1-8+10 for shoe, and at 1-9+15 for airplane. About all these cases we could say, if we wished, that the child was extending his use of a sound in an extraordinary and striking manner: to mean two different pleasant smells, or to mean milk, cake and butter; or to mean airplane and shoe; and this is in fact is what such an observer as Idelberger says of his son's varied uses of the sound **ɑ,** a list which is cited by Lorimer (8) as an instance of the unstable character of early "nominal relationships."

But if we trace the child's development as I have done here it is clear that a sound such as **ɑ** or **e** is not a name for any one object, subsequently extended to another. It is simply a differentiation of the child's earlier use of a sound to express an affective state, a use which gradually becomes more definitely directed to the situation with which he is dealing, without however being attached to any one particular situation. And if we accept this, then it also becomes clear that when the child begins to use a conventional word such as **fɑ** for flower instead of his own primitive sounds, he is at first likely to use it just in the same extended manner as he has been using his own sounds. This point needs to be stressed, because when we find a child using adult words it is only too natural for us to imagine that he is using them as names for particular objects; but more careful observation makes it clear that he is using them not very differently from the way in which he was using his own primitive sounds not long since.

For instance, if we heard a child who has been accustomed to say ɑ on seeing flowers, say this the first time he sees a branch of flowering cherry, we should probably regard this simply as an expression of the child's delight at seeing the beautiful thing, and of his desire to touch and smell it. It would hardly occur to us to say that the child has made a generalization, extending the name ɑ from its use for tulips and applying it now to cherry-blossom. But let us suppose that this happens at a time when—as in the case of K—he was already using the word **fɑ**; at once we feel that the child has shown himself capable of generalization, that he has realized the similarity between the tulips and the cherry-blossom, and therefore uses the same name for them both.

Clearly we can easily overestimate the difference between these two stages—before and after the child adopts the conventional word. It is true that there has been some development: The word **fɑ**, because it has been adopted from the adult name for the flower, is rather more closely attached to that object than the child's own sound ɑ had been. But even so, the use of a word, first for tulips and then for cherry-blossom, cannot be accepted purely as a case of objective reference to one thing, followed by extension of the name to another thing on a basis of objective similarity. For in neither case is the child using the word simply as a means of naming a thing, as a label to be attached to an object. Just as when the child said ɑ ɑ he was at the same moment drawing attention to the flower, expressing his delight at seeing it and possibly his wish to smell it, so now when he uses the learnt word **fɑ**, this new utterance must retain something of the new functions of the earlier sound which it replaces.

Further, it is also brought home to us that this extension of the use of a word to apply to a new situation is something more than the evocation of a sound from a passive child. It is the child's own activity which brings about the extension. The flower which he sees is something to be grasped and smelt. And the word he utters is a means by which the attention of others may be drawn to this desirable thing (the declarative use), or their help secured in obtaining it (the manipulative use). Of course, it need hardly be said that I am not suggesting that the child will be aware of these functions; only that they are likely to be present in a more or less rudimentary form. What I *am* suggesting is that because the word and the new situation each plays a part in the child's behavior he will broaden his use of the word to apply to the situation.

Thus in tracing the course of the child's development preceding his adoption of conventional words, we see that objective extension is not to be regarded as beginning with this adoption, but that it is based upon the early use of a sound to express an affective state, first in a wide range of situations, and then with an increase of objective reference to these situations. This use develops under the stress of the child's need to bring other human beings into the situation, to obtain their help, or at least their company. Then, by the intervention of these other human beings, the original widely-functioning sound-group is replaced, in one situation after another, by a specific conventional sound-group. We next have to consider which features of these situations are likely to give rise to wide applications of words.

THE KINDS OF SIMILARITY AMONG SITUATIONS

Objective Similarity. Stern (10) gives a comprehensive summary of the objective conditions under which the application of a word will be extended. He classifies the conditions of similarity between situations into three groups. First, the situations may be similar as a whole; second, there may be similarity of particular features; third, the child's attention may shift from feature to feature, so that a word is applied to a new situation which has no resemblance at all to the original situation, but has some point of likeness to an intermediate situation.

1. *Similarity of the Situation as a Whole.* In Stern's first group of cases a word is applied from a customary situation to another one which as a whole objectively resembles it; for instance, **pei** for an airplane and then for a kite, **mooi** for the moon and then for any round object. Many other instances could easily be given.

2. *Similarity of Particular Features.* Stern's second group of cases are those in which the likeness is said to exist only in particular features of the situations. This may be illustrated by two cases from my own observations. The first is K's use of the word **kotibaiz.**

At 2-2+14 he was given a large toy abacus; immediately on seeing it he said **kotibaiz.** It was impossible to tell whether the use of this word was due to a memory of a cot in which he had last slept more than a year since, and which had one or two rows of beads, or whether he was referring to the parallel wires of the abacus which resembled the parallel bars of his own cot. Two months later, at 2-4+12, he used the same word **kotibaiz** for two other objects: on first seeing a toast-rack (with parallel bars), and again for a picture of a building with fluted columns.

In the second case, having learnt to say **fi:** (sea) for the seashore, K subsequently said it on seeing a railway track; there was certainly a resemblance between the slope of the grassy sand banks by the sea and the slope of the railway embankment.

3. *Shifting from Feature to Feature.* Stern's third group of cases are those in which the extended application of a word is due to particular features similar to those present in an earlier situation. Here Stern cites the instance from Romanes which we have already mentioned, where the word *quack* first used for a duck, ultimately came to be used for a coin. Similar cases are recorded by Idelberger (6); the child, having used **wauwau** on seeing a dog or hearing the animal bark, later said it at the sight of a fur collar with a dog's head, then for a fur collar without a head, finally for buttons; presumably he had seen buttons on the fur collars. It was the same child who used the word **baba** both for his father and for the sound of a bell; the route this time being from his father to his father's trousers hanging on a hook, then for his father's portrait, then for an electric bell which his father had repaired, finally for the sound of any bell.

This threefold classification of Stern's certainly helps us to understand what we may call the objective side of the process—the various kinds of objective similarity among situations which may help to bring about the application of a word from one to another. But when we come to examine both Stern's own cases and others more closely, we find that this analysis is inadequate.

THE AFFECTIVE ASPECT OF EXTENSION

In the first place, Stern hardly lays sufficient emphasis upon the part played by the child's affective responses. It is true that he mentions affect as a factor which is likely to permeate all the varied applications of a word, but he makes no further reference to it in his detailed analysis. This neglect seems strange in the light of the importance that has been given to affect as one of the chief factors in determining the influence of past upon present experience, not only by the psychoanalysts but by English writers such as Ward, Stout, and Bartlett; and all the more strange since Meumann—to whom Stern constantly refers—paid special attention to this point in his account of children's wide applications of words.

Meumann's view is this (9): That in cases where situations are only very slightly similar objectively, extension of a word may take place because the situations arouse in the child similar feelings and similar strivings. To apply this, for instance, to a case given by Stern; his daughter Hilde at 0-11 extended the word **puppe** from her doll, to a toy rabbit and other playthings. Stern suggests from this that a general similarity of the most superficial kind may be enough to bring about extension; but if we follow Meumann here we should say that the sight of a toy rabbit aroused the same feeling of delight and desire to play as the original doll had aroused; and it was this affective-conational similarity which was powerful in bringing about the extension.

The truth is perhaps that Stern, making a somewhat conservative summary, is still mainly concerned with the cognitive factors in the process of extension, and sees this process mainly as a step in the development of abstract thinking; but there is no doubt that Meumann was right: The more we pay attention to the conditions in which the wide applications of words take place, the more certain does the place of affect become. It plays an important part, as we have seen, while the child is still using his own primitive sounds, and it retains this function when he comes to replace them by conventional words. When, for instance, K said **fi**: for the first time it was not a mere indication of the sea or sandhills; the child had been awakened by the thunderstorm and called out the newly learnt word to express his terror at what he thought was the roaring of the sea. In the same way when he said **pei**, first for an airplane and then for a kite, it was surely partly because he felt the same delight (or perhaps fear or wonder) at them both. A few weeks earlier he would have said **e e** on seeing the new thing; now he uses the learnt word **pei** which, whatever else it may do, expresses his affective reaction just as his earlier sounds had done. Again, when he said **fɑ** on seeing the cherry-blossom, this must have expressed the peculiar pleasure aroused in him by the sight of a flower. Observations of this kind make it certain that affective similarity may be a very powerful factor in determining the wide use of words.

FUNCTIONAL SIMILARITY

If affective similarity has been neglected, this is even truer of functional similarity. Yet it is more than 40 years ago that Dewey first drew attention to its importance, in his usual vigorous fashion (4).

> The tendency to apply the same term to a large number of objects ("ball" to ball, orange, moon, lamp-globe, and so on) can be understood, I think, only if we keep in mind the extent to which the formal noun "ball" has really an active sense. "Ball" is "to throw" just as much as it is the round thing. I do not believe that the child either confuses the moon with his ball, or abstracts the roundness of it; the roundness suggests to

him something which he has thrown, so that the moon is something to throw—if he could only get hold of it.

In a word, Dewey insists upon the importance of functional similarity in the situations for the child, that they do not merely exist statically for him, but are bound up with his activity. To this we shall have to add that they are also frequently bound up with the activity of those around him. Functional similarity of either kind may determine the wide use of words.

THE FUNCTION OF THE SITUATION IN THE CHILD'S OWN ACTIVITY

This point is not absolutely neglected by Stern; he does note in passing what he calls an exceptional instance where the function of the situation becomes a factor in bringing about the wider use of a word. Hilde, he tells us, used *Nase* at 1-7 for the toe of a shoe, because she found she could pull this in the same way as she would playfully pull someone's nose. But we have to go further than Stern and point out that this is by no means an exceptional instance; functional similarity is as constant a factor as objective or affective similarity.

Take, for instance, Hilde's use of **puppe** for a wide range of playthings, including a doll, a toy rabbit, and a toy cat; Stern recognizes that these things are objectively unlike each other, and urges therefore that extension will occur when only a very superficial resemblance is present. But the fact is that a stuffed cat is really very unlike a doll, unless we remember that a child *plays* with both of them. Again, to go back to K's use of **fɑ** for tulips as well as cherry-blossom; the resemblance is certainly very superficial unless we remember that when he saw a flower he was usually invited to *smell* it.

It is equally necessary to emphasize this functional factor in these cases where the likeness is said to lie not in the situations as wholes, but in particular features of them. When K used **fi:** for the railway track after having used it for the seashore, there can be no doubt that the word was evoked not only by the resemblance between the two slopes, but also by the fact that the child had enjoyed climbing the sandbank and that here was something else to climb. That this view is justified is shown by an observation of mine in this connection; a few days after the episode of the railway track the child was taken near a grassy bank; this time he said **ʌpi ʃi:**, using again the word **ʌpi** that he had constantly used when he wished to be taken over the sandbanks by the sea.

It is interesting to notice that in some cases, the wide application of a word seems to rest almost entirely upon the existence of this functional similarity. Take, for instance, K's use of the word **dɑ** (from the word *down;* at 1-6+8) he said the word before climbing *down* from his mother's knee; a few days later he began to use the word frequently to indicate that he wished to go *up* stairs (1-6+16-26). Here the circumstances show that the point of similarity was not a matter of direction, up or down, but that there was climbing to be done in both cases.

THE FUNCTION OF THE SITUATION IN THE ACTIVITY OF OTHERS

In a further group of instances we have to take a broader view of this notion of functional similarity; where the situations have a similar function, not in the child's own activity, but in the activity of some other person. For instance, among the extraordinary uses made by Idelberger's son of the word **wauwau** were those which included, first, a sewing table and secondly a bathroom thermometer. Now the only possible point of resemblance between these two objects is this, that they are instruments used by someone. Again, Guillaume tells us (5·) that at 1-2 his son learned the word **ɑto** (*marteau*) for hammer, and in a short time was applying it to a large variety of objects: button-hook, hand-mirror, comb, handbag, saucepan, hairpin, wooden spade, key, gun, box, belt, purse, ruler, puttees, basin, safety pin, candlestick, coffee-mill, plate, spoon. Guillaume adds that it was never used for people, animals or food, but seemed to be equivalent to *machin* or *chose,* that is to say, an instrument used by some person.

No interpretation which relies upon the evocation of a word by the similarity of objective features can cover a case of this kind. We are bound to say that the word is the child's way of dealing with a complex situation, the similarity of which with another situation is now mainly this, that in each case someone is using something, a person manipulating an instrument.

It is clear that to Stern's emphasis upon objective similarity we have to add the two other features emphasized by Meumann and by Dewey—affective and functional similarity. Together these three views give us a comprehensive view of the similarities of the *situations* among which a child may extend the use of a word. Now if we look at this summary two very interesting points stand out. The first is that while the traditional account emphasized by Stern gives a picture of a somewhat passive child from whom a word is wrung on successive occasions by similar situations, both Meumann and Dewey emphasize the place of the situation in the child's activity. And secondly, that all these three views alike deal only with one side of the process—the nature of the situations—and neglect the other side—the nature of the instrument which is applied to those situations, that is to say, the function of the word used.

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Four—The Development of Language in Twins

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Most studies of language development have been naturalistic. The child's vocalizations have been described, his progress has been charted, but seldom has the investigator introduced any experimental factor to determine its influence upon the child's language.

In the study described in this section, there is an experimental factor–twinning. There is no reason to believe that twins in general differ in heredity from singletons in general. Differences between twins and singletons in language may therefore be attributed to the unusual social situation created by the presence of two children of the same age.

INTRODUCTION

In the year 1886, Horatio Hale (17) in a paper published in the Proceedings of the American Association for the Advancement of Science, reported the case of a pair of twin boys who were so retarded in speech that members of the family could not understand them. These children lived in Boston in 1860. According to the report of an aunt, who visited them frequently, they developed a twin language so unlike English that they could not be sent to school because no one could understand them. Observation of the occasional development of such a special language by twins, suggested the problem of research undertaken here. The twin situation is, from an environmental point of view, quite unlike that of single children and it seemed possible that this might be a factor of considerable significance in the development of language.

McCarthy's (13) investigation *The Language Development of the Preschool Child* provided an excellent basis for comparison. This study was completed in the summer of 1928 at the Institute of Child Welfare of the University of Minnesota. Dr. McCarthy selected 140 children (20 at each six-months-age-level from eighteen to fifty-four months) on the basis of sex and socio-economic status. She visited them and recorded 50 consecutive responses made by each child while he played with a group of toys that she presented to stimulate speech. These data were then analyzed in four different ways: by mean length of response; according to the Piaget functional analysis; by the grammatical construction of the sentence; and a word analysis by parts of speech. In brief the results indicated the developmental changes with age in agreement with other studies, as well as sex differences in favor of the girls. Differences in favor of the upper occupational groups were indicated also in each method of analysis. In the foreword to this study Dr. John E. Anderson says "On the basis of the number of controls utilized in the selecting of subjects and the care and thoroughness with which the data are analyzed, this monograph stands out as one of the best studies of the development of language which has yet appeared." The present investigation was planned to repeat the McCarthy technique using twins.

PURPOSE

The purpose of this investigation was to compare the development of language in twins with that of singletons of the same ages, sex, and socio-economic status.

SELECTION OF CASES

Through the courtesy of the Minneapolis and St. Paul Departments of Health, the writer had access to the birth registrations. The date of birth, sex, place of birth, name of physician attending the birth, and the father's name, address, and occupation were all recorded on the registration cards.

SUBJECTS

Age and Sex

After consideration of the findings of the McCarthy study, it was decided to select 80 pairs of twins, 20 pairs at each of four age levels, two, three, four, and five years. Since McCarthy reports that 74 per cent of the responses of the eighteen-month-old children were incomprehensible, it seemed adequate from the standpoint of the methods of analysis to be used, to begin with the two year age group. The wisdom of carrying the group on through the fifth year was not so apparent at the time the selection was made as the results now show it to be. An equal number of boys and girls at each age were selected except at age four (see Table I) where there are 19 boys and 21 girls. These age groups are discrete because the records were taken within one month of the birthday. With the exception of one case where the records of the two children were not taken on the same day, the greatest deviation from the birthday was 38 days and the mean deviation for the whole group was 19.69 days. The one exception referred to deviated 99 days from the birthday.

Socio-economic Status

The importance of securing a representative sampling of the population is clearly brought out in such studies as the McCarthy study and the Goodenough (9) evaluation of the Kuhlman-Binet tests for children of preschool ages. The sampling was made according to the Goodenough (10) classification of the Minneapolis population which was based on the application of the Barr Scale for Occupational Intelligence and the Taussig industrial classification to the 1920 census report of the occupations of males in Minneapolis between the ages of twenty and forty-five years. The occupational levels are grouped into sixfold classification. Group I consists of the professional vocations; group II the managerial occupations; group III the retailers, salesmen, and skilled workmen; group IV the semiskilled workmen; group V the drivers, helpers, and the like; group VI the day laborers. Table 1 shows the percentage of each occupational class found in the Minneapolis population and the percentages represented by twins and singletons.

The distribution of the sexes is not equal at each occupational class but is equal in the upper three and lower three classes at each age, except at age four.

METHOD OF OBSERVATION

The Language Record

Just as the selection of cases was made on a basis similar to that used in the McCarthy study, the technique of obtaining the language record was carried out as it was with the singletons. A group of toys consisting of two linenette picture books, one of

TABLE I

Distribution of Cases by Age, Sex, and Occupational Class

Occupational Class	2 Years		3 Years		4 Years		5 Years		*All (number of pairs)*	*Twins*	*Minneapolis Population*	*McCarthy Singletons*
	Boys	Girls	Boys	Girls	Boys	Girls	Boys	Girls				
I	1	1	2	0	0	2	1	1	4	5.0%	5.4%	5.0%
II	2	0	1	1	0	2	0	2	4	5.0	6.3	6.4
III	7	9	8	8	10	6	9	7	32	40.0	37.3	35.7
IV	6	4	4	6	4	6	3	5	19	23.8	24.3	25.7
V	2	4	3	3	1	5	3	5	13	16.2	14.9	15.7
VI	2	2	2	2	4	0	4	0	8	10.0	11.8	11.4
Total	20	20	20	20	19	21	20	20	80	100.0%	100.0%	100.0%

familiar animals and one of Mother Goose, a very small automobile, a rubber ball, a pasteboard cat that meowed when squeezed, a telephone with a bell, and a small music box were shown to the children to stimulate speech. In so far as possible, McCarthy's toys were duplicated exactly. The examiner requested to see each child alone but this was not always possible. The children were permitted to open the suitcase and look at the toys as they wished. The examiner remained out of the situation as much as possible because spontaneous responses were desired. Some conversations between the examiner and the child however always occurred. When members of the family insisted upon remaining in the room, they sometimes facilitated spontaneous speech on the part of the twin and sometimes interfered with obtaining an accurate record.

Fifty consecutive verbal responses were recorded and the time noted. The response were recorded exactly as they sounded to the examiner. A response was denoted as complete when a distinct pause occurred. A complete sentence was always considered as a single response, although a single response might be either a complete or incomplete sentence. The mean time for obtaining such a record was 14.9 minutes for the twins as compared with 19.3 minutes for the singletons. The shorter time required by the twins is probably due to two factors. In the first place, they said considerably fewer words in the 50 responses. In the second place, the examiner was in the home anywhere from one to two and a half hours and at least one of the children had a somewhat greater opportunity to become adjusted to the presence of a stranger. The examiner practiced taking such records with a group of children attending the nursery school at the University of Minnesota, preliminary to taking any records with the twins. She also had the advantage of being able to talk with Dr. McCarthy frequently and so check on minor differences in procedure.

The Intelligence Tests

The Goodenough (18) drawing test of intelligence was given by the examiner to all the twins. This test consists merely in having the child draw a man. It is nonverbal and very simple to give. The results however were not usable since such a large percentage of the twins had not manipulated pencil and paper before and therefore they only scribbled.

The department of mental testing of the Institute of Child Welfare volunteered to give the twins the Minnesota Preschool Scale (11), which at this time was in the process of standardization. This test was devised by Goodenough and is a revision, for the most part, of the tests for these ages in the Kuhlman-Binet and the Stanford-Binet tests. Test results were obtained on 123 of the 160 children.

METHODS OF ANALYSIS

McCarthy's methods of analysis were duplicated exactly. A total of 7,836 responses were made and of these 7,156 or 90 per cent were comprehensible and 780 or 10 per cent were classed as semicomprehensible or incomprehensible.

Length of Response

The comprehensible responses were analyzed first according to length of response. McCarthy set up the following arbitrary rules for this analysis:

1. Contractions of subject and predicate like "it's" were scored as one word.
2. Contractions of the verb and negative like "can't" were scored as single words.
3. Hyphenated words and compound nouns functioning as single words like "Mother Goose" were scored as one word.
4. Each part of a verbal combination was scored as a separate word.
5. "Lookit" which occurred frequently was scored as one word if used alone, although if followed by an object it counted as two words.

RELIABILITY

Method of Observation

In order to get some estimate of the degree of accuracy in recording such a verbal record, Dr. McCarthy, the examiner, and one other observer took records simultaneously, 25 responses in length, on 10 two-to-five-year-old children from a settlement house.

Table II shows the coefficients of reliability found between each two observers. The examiner first analyzed the language records as to length of response, the functional classification, and the construction of sentence category, just as the original data had been treated. The Spearman method of rank differences was used to determine the coefficients of reliability. In length of response the mean for each child according to each observer was used for the correlation and in the other analyses the percentage of whatever item was being correlated was used.

These coefficients seem sufficiently high to indicate reliability of the method of recording.

TABLE II

Reliability Coefficients on the Method of Recording

	Observation			
	1 and 2	*1 and 3*	*2 and 3*	*Mean of All*
	ρ	ρ	ρ	ρ
Mean length of response	.98	.99	.99	.99
Naming	.90	.99	.94	.94
Remarks about immediate situation	.95	.87	.94	.92
Functionally complete but structurally incomplete	.92	.91	.99	.94
Simple sentences	.32	.69	.72	.57
Incomplete sentences	.65	.40	.87	.64

Methods of Analysis

The reliability of the various methods of analysis used in treating the language records was not determined in this investigation since McCarthy had determined this and found it to be high.

COMPARISON OF DATA ON LENGTH OF RESPONSE: TWINS AND SINGLETONS CHRONOLOGICAL AGE

Figure 1 shows a surprising difference in rate of language development for twins and singletons. At two years the difference is small but appears to become increasingly greater with age and is consistently in favor of the singletons. The mean length of response for the five-year-old twins is slightly below that of the three-year-old singletons. A retardation of two years in language development, one would think, was a serious handicap since contacts with our fellow-

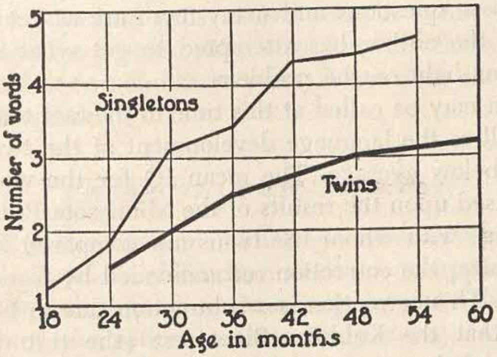

Figure 1. Mean length of response of singletons and twins by chronological age.

men depend so largely upon our ability to make ourselves understood verbally.

Critical ratios $^{D}/\sigma_{D}$, computed for each age level were high and increased with age. At two years the ratio was 4.9; at three, 11.6; and at four, 15.3. Hence the superiority of singletons to twins in language development is highly significant statistically.

Smith's investigation (18) of the development of the sentence and the extent of vocabulary in young children agrees very closely with the McCarthy results, and so only further emphasizes the handicap of the twins. The most rapid gain in length of response is made between two and three years by both twins and singletons. Although twins gain only one word in the entire year, singletons gain more than one word in the first half of the year. This difference in rate of development remains relatively the same up to five years. Smith feels that the relatively little gain in length of sentence after four or four and one-half years indicates that the sentence length has little value as a measure of sentence development after this age.

The question immediately arises as to when the curve of development for the twins turns and reaches the level of that of the singletons. Does the greater contact with other children, which occurs upon entrance in school, at or soon after the fifth year, stimulate language development sufficiently to give the twins this needed spurt? Does the twin situation so limit the child's environment in the early years of rapid development that he is always at a disadvantage? What is responsible for the retardation, heredity or environment, or both?

All of these questions and many more are as yet unanswered. Nevertheless the author has attempted to get what evidence she could to throw light on the problem.

Attention may be called at this time to the fact that the mental ability as well as the language development of the twin group appears to be below average. The mean IQ for the whole group is 94. This is based upon the results of the Minnesota Preschool Scale. The singletons, with whom the twins are compared have a mean IQ of 103.3, after the correction recommended by Goodenough has been applied. This correction is a subtraction due to Goodenough's observation that the Kuhlman-Binet test (the test used for the singletons) tended to score too high at the early ages. The singletons then appear to have been a group slightly superior even though they were so carefully selected. Had they been an average group the differences would be slightly decreased. That the twin group deviates even farther from normal is quite true although the evidence seems to be that this is a normal phenomenon rather than a matter of selection.

SEX DIFFERENCES

In agreement with the findings of McCarthy, Nice (15), Gale (6), Mead (14), Terman (20), and Doran (4), this reveals sex differences in favor of the girls. (See Figure 2.) These differences are less marked than in the group of singletons at every age except four years, but are consistently in the same direction. It may be noted that at two years the twin girls are slightly superior to the single boys. The twin boys make a greater gain between two and three years than do twin girls but gain less thereafter. The differences between twin boys and girls at five years is greater than at any other age, while the greatest differences between the sexes in singletons was found at two and three years. Smith does not report a consistent sex difference.

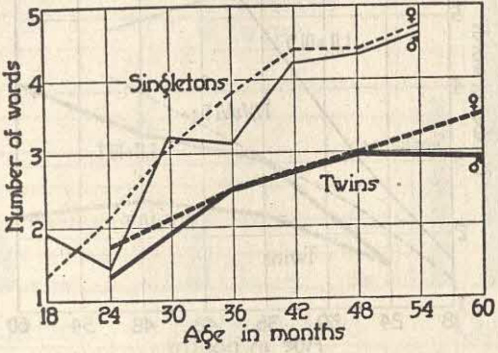

Figure 2. Mean length of response of singletons and twins by sex and chronological age.

PATERNAL OCCUPATION

Because of the unequal number of cases in each of the six occupational classes the differences between occupational classes is most fairly shown by dividing the age groups into the upper three and lower three occupational classes. By so doing an equal number of cases and an equal number of each sex appears in each group. Figure 3 shows the expected differences between these groups. In comparing these results with the McCarthy results it must be noted that the age groups are not strictly comparable. The singletons were so grouped in reporting these findings that the age groups become eighteen, twenty-seven, thirty-nine and fifty-one months, rather than at the exact year levels of the twins. The differences between twins and singletons are even more striking when based upon distribution by occupational class. That the singletons of the lower occupational groups are consistently above the twins of the upper groups presents

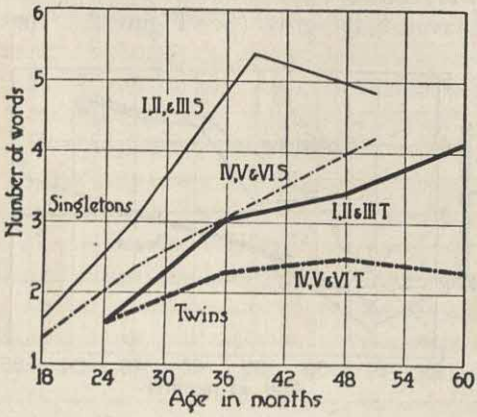

Figure 3. Mean length of response of singletons and twins by upper and lower occupational groups and chronological age.

a surprising picture of retardation. The mean of the differences at each age between upper and lower groups is .90 words for the singletons, .85 words for the twins so that the differences between upper and lower groups remain relatively the same. The fanning out of curves of the upper and lower group twins indicates that the difference increases with age, which suggests that environment plays a rôle in this.

COMPARISON OF DATA ON FUNCTIONAL ANALYSIS; TWINS AND SINGLETONS

The percentage of responses of the functional classification were worked out both inclusive and exclusive of the incomprehensible responses. Figure 4 shows the percentages of this classification based on all responses, including the incomprehensible responses.

When considered from the standpoint of age groups, the incomprehensible responses, though somewhat fewer for twins at two years are slightly greater at three and four years and are still in evidence at five years. With the marked retardation in language development indicated by the differences in mean length of response of twins and single children, one might expect a very much greater percentage of incomprehensible responses on the part of the twin. Such responses do seem to persist for a longer time in the twins, however, which is suggestive of a slower development. Two factors in the twin situation may have acted to reduce somewhat the percentage of incomprehensible responses. It may be easier to understand short statements as compared with long ones and the twin data are composed of many more single word responses. Also, the examiner had had, just previous to making this study, two and one-half years' nursery school experience and was quite accustomed to interpreting the speech of young children. Dr. McCarthy on the other

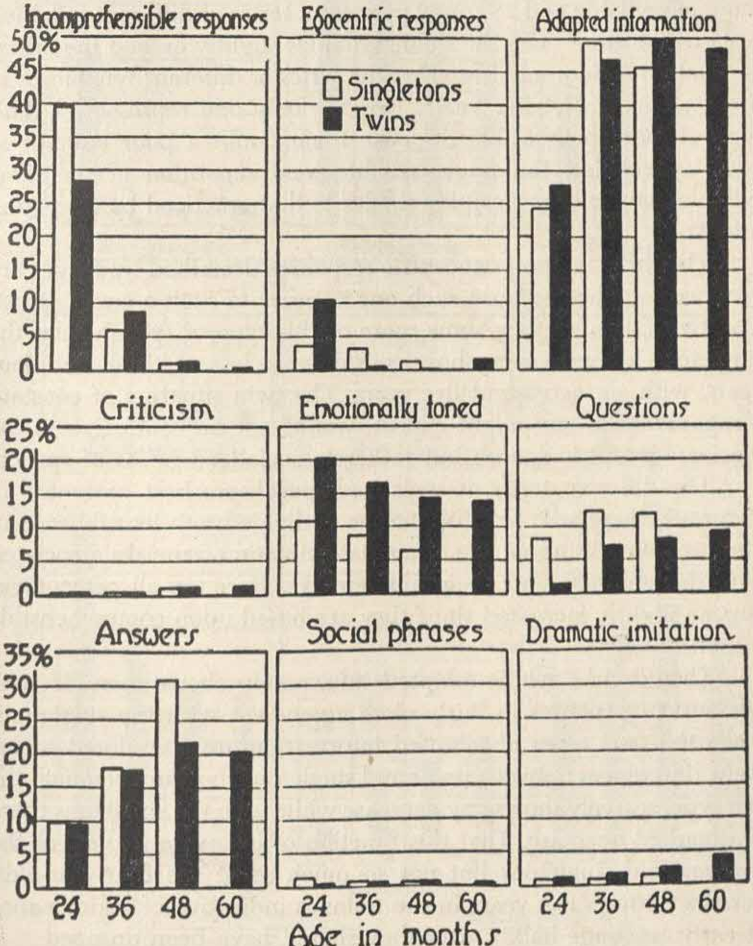

Figure 4. Functional analysis of singletons and twins by chronological age.

hand had had only the contact any adult may have with children previous to collecting her data.

The 780 incomprehensible responses include 83 semicomprehensible responses. Of 697 responses actually incomprehensible 287 or 41 per cent were single syllables, 121 or 17 per cent repetition of the same syllable and 289 or 42 per cent a series of different syllables. At two and three years the single syllables slightly exceed the others in number. At four and five years the series of different syllables is in the lead, in fact at five years, it is the only one represented. This type of incomprehensible response is no doubt a poor attempt at adult speech but the single syllables and repetition of the same syllable may be the voice play similar to that practiced by the young infant.

The three types of egocentric responses described by Piaget are grouped together because each one appears to such a small extent. The twins, though they show more of this type of speech than the singletons at two years, show considerable less at three and four years, with an increase at five years. The twin situation of constant companionship, one might expect, would not be conducive to extensive egocentric speech but rather to socialized forms of speech.

The different types of socialized speech are best presented in Figure 5 where adapted information is divided into its subheads of naming, remarks about the immediate situation, remarks associated with the situation, and irrelevant remarks. Here too all percentages appear slightly increased since they are based upon comprehensible responses only.

Though as a whole adapted information shows increase with age and percentages in fairly close agreement with the singletons, when the four types of adapted information are considered separately, differences between twins and singletons show up. Naming, the first type, scarcely shows any decrease while with the singletons there is a marked decrease. That this function of language, typical of the two-year-old singletons but not so much so of the four-year-olds, persists through five years in the twins is indicative of maintenance of early language habits after they should have been dropped.

Remarks about the immediate situation increase with age but are present in greater numbers in twins than in singletons, whereas remarks associated with the situation are very few in number as compared with singletons, show relatively no increase with age, and do not appear at all until three years. In consideration of these two differences, one may wonder whether the limitations of the twin environment may account for these differences.

The percentages of irrelevant remarks are comparatively in-

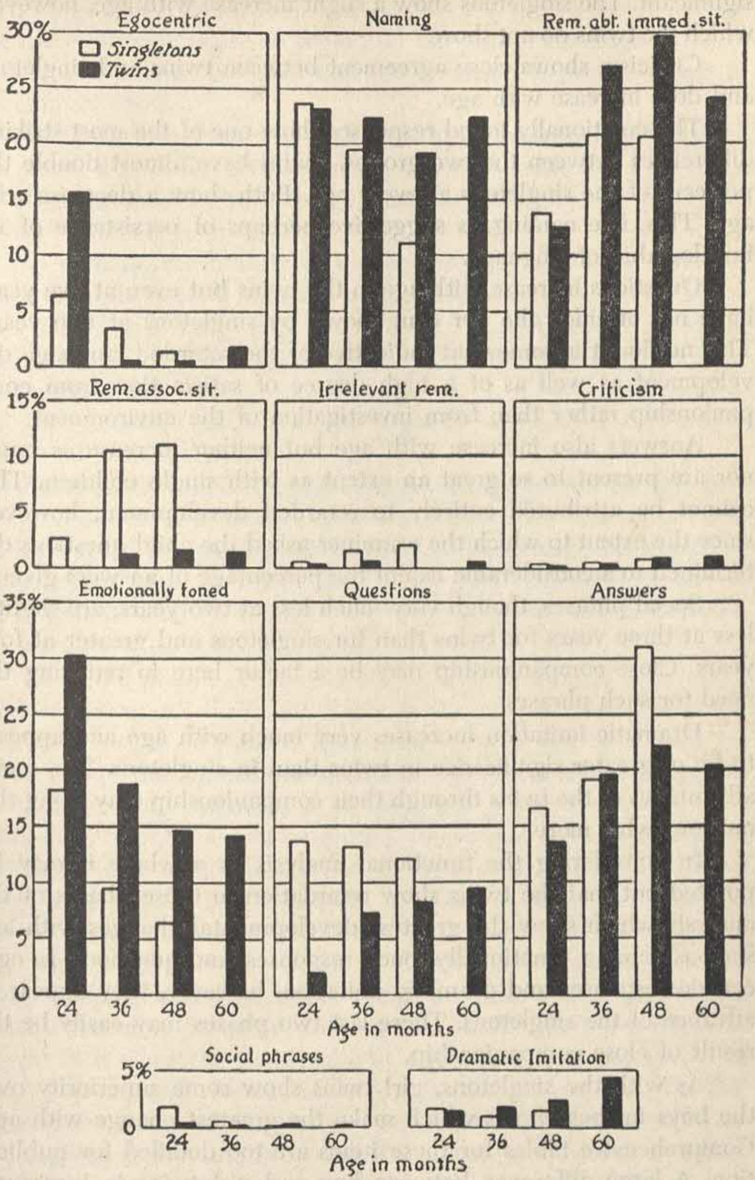

Figure 5. Functional analysis of singletons and twins by chronological age (based on comprehensible responses).

significant. The singletons show a slight increase with age, however, which the twins do not show.

Criticism shows close agreement between twins and singletons and does increase with age.

The emotionally toned responses show one of the most striking differences between the two groups. Twins have almost double the per cent of the singletons at every age. Both show a decrease with age. This, like naming, is suggestive perhaps of persistence of infantile habits of language.

Questions increase with age in the twins but even at five years have not attained the per cent shown by singletons at two years. This no doubt is somewhat indicative of the retarded language development as well as of a high degree of satisfaction from companionship rather than from investigation of the environment.

Answers also increase with age but neither increase as much nor are present to so great an extent as with single children. This cannot be attributed entirely to retarded development, however, since the extent to which the examiner asked the child questions determined to a considerable extent the percentage of answers given.

Social phrases, though very much less at two years, are slightly less at three years for twins than for singletons and greater at four years. Close companionship may be a factor here in reducing the need for such phrases.

Dramatic imitation increases very much with age and appears to be of greater significance in twins than in singletons. The social advantages of the twins through their companionship may bring this out somewhat more.

In considering the functional analysis as a whole it may be pointed out that the twins show retardation in those phases of the analysis which show the greatest developmental changes with age such as naming, emotionally toned responses, and questions. In egocentric responses and dramatic imitation, however, they appear in advance of the singletons. These last two phases may easily be the result of close companionship.

As with the singletons, girl twins show some superiority over the boys in those items which make the greatest change with age. Comprehensive tables for these items are too detailed for publication. A large difference between boy and girl twins in incomprehensible responses at two years puts the girls quite in advance of the boys. At three and four years the girls have a slightly greater percentage of incomprehensible responses, and at five they have less but these differences are so slight and the total percentage at three, four, and five years so small that the reversion is probably due to selection of cases.

The girls also show less egocentric speech at every age except five years. The differences are small, however, except at two years, which is the most significant age from the standpoint of total percentage of this type of response.

In naming, the boy twins increase slightly while the girls decrease from the second to fourth year and then increase at five years. Girls have somewhat greater proportion of remarks about the situation and both boys and girls increase with age to four years then drop a bit at five years.

Girls increase more and are superior at all ages in remarks associated with the situation.

Emotionally toned responses are decidedly greater for the girls except at age three where a difference of only 1 per cent is in favor of the boys. The greatest differences in percentage of questions occurs at two years and is in favor of the girls. The other phases of the functional analysis though they show some differences between the sexes show no striking or consistent trends.

The chief phases of analysis which showed marked differences between the upper three and lower three occupational classes are incomprehensible responses, egocentric speech, questions, and remarks about the immediate situation. The greatest difference in incomprehensible responses is at two years where the upper class twins show about one-half as great a proportion as the lower class. Just the reverse situation is found in egocentric speech. The lower group have slightly less than twice as much as the upper group at two years, and have none at all at the other ages. This was also the case with the singletons, except at thirty-nine months, where the lower group exceeded the upper group by quite a large proportion. Perhaps the sampling is not sufficient for these differences to mean a great deal since the percentage at any age is not very great. In both questions and remarks about the immediate situation the differences are not so marked at two and three years but become increasingly so at four and five years. The lower group shows a smaller proportion of these than the upper groups. A similar situation was found with singletons in regard to questions but not in regard to the other phase.

COMPARISON OF DATA ON CONSTRUCTION ANALYSIS: TWINS AND SINGLETONS

Chronological Age

Figure 6 again shows that twins are retarded in language development as compared with singletons. Since they were found so retarded when measured by length of response one could scarcely expect to find them using many of the more complex forms of the sentence. It may be said, however, that in general the same tendencies with age are indicated in both the twins and singletons.

The functionally complete and structurally incomplete responses though they decrease with age are present to a somewhat greater extent in twins than in singletons. This group as has been stated, consists very largely of the single-word responses. Although such responses appear rather commonly in adult speech they appear there largely in conversational usage while the young children employ them in other ways. For instance, in looking through the picture books it was not uncommon for the children to point at and name objects, and use no sentence at all. Twins evidently maintain this rather infantile manner of talking about what they see longer than do single children.

The simple sentence and all other forms of complete sentences increase with age but at every age are present to a considerably less extent than they are in singletons. The more complex the form of the sentence the greater the relative differences between twins and singletons become.

Incomplete sentences while not always present to a greater extent than in singletons do not drop off as rapidly. This again indicates persistence of early modes of talking or of rather infantile language habits.

The sex differences in sentence structure are not as marked for twins as singletons. There is no consistent trend in the differences between the boy and the girl twins in any of the items of the classification. It may be noted, however, that the girls do show all the forms of the complete sentence earlier and in larger proportion than the boys. Probably these items show the most important developmental changes with age and thus the girls may be considered in advance of the boys since they are using the more complex forms of the sentence earlier.

In relation to the singletons both boy and girl twins are retarded. In fact, as in the length of response, the girl twins rank below the boy singletons in practically all classifications of construc-

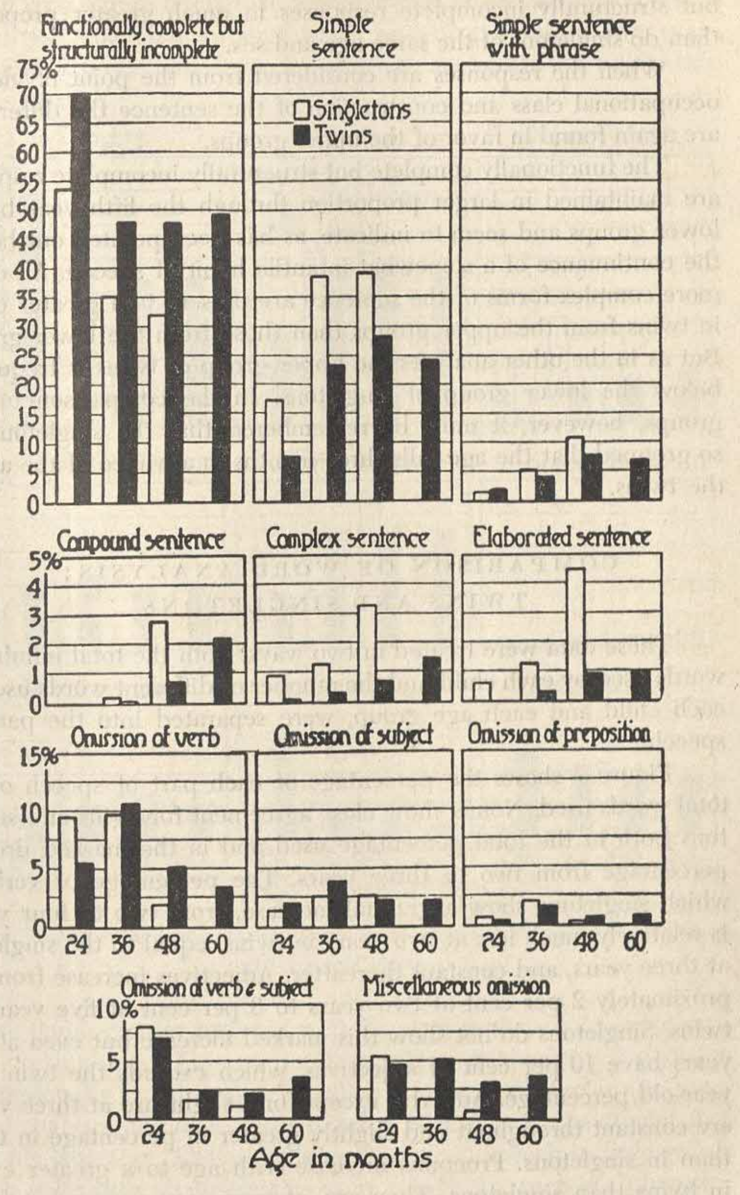

Figure 6. Construction analysis of singletons and twins by chronological age (exclusive of incomprehensible responses).

tion. Both sexes show the continuance of the functionally complete but structurally incomplete responses in much greater proportion than do singletons of the same age and sex.

When the responses are considered from the point of view of occupational class and construction of the sentence the differences are again found in favor of the upper groups.

The functionally complete but structurally incomplete responses are maintained in larger proportion through the fifth year by the lower groups and seem to indicate, as has been pointed out before, the continuance of a somewhat infantile habit of speech. All of the more complex forms of the sentence are present to a greater extent in twins from the upper groups than those from the lower groups. But as in the other analyses the upper group of twins is frequently below the lower group of singletons. In the comparison of two groups, however, it must be remembered that the singletons are so grouped that the age falls three months in advance of the age of the twins.

COMPARISON OF WORD ANALYSIS: TWINS AND SINGLETONS

These data were treated in two ways. Both the total number of words used by each child and the number of different words used by each child and each age group, were separated into the parts of speech.

Figure 7 shows the percentage of each part of speech of the total words used. Nouns show close agreement for twins and singletons both in the total percentage used and in the marked drop in percentage from two to three years. The percentage of verbs in which singletons show a gradual increase, from two to four years, is relatively much less at two years in twins, equal to the singletons at three years, and constant thereafter. Adjectives increase from approximately 2 per cent at two years to 8 per cent at five years, in twins. Singletons do not show this marked increase but even at two years have 10 per cent of adjectives, which exceeds the twin five-year-old percentage. Adverbs, except for a slight use at three years, are constant throughout and slightly greater in percentage in twins than in singletons. Pronouns increase with age to a greater extent in twins than singletons. They are, at every age, somewhat fewer in proportion. Conjunctions are late in appearing. Girl singletons show a small per cent at eighteen months and the boys show none until thirty months. A smaller per cent (.9) appears at three years in the twins, and at five years the proportion is still smaller than that for singletons. Prepositions, although they show a greater increase with age in twins than in singletons, at two are much less in proportion, the same at four years, and drop some at five years. Although interjections decrease with age markedly, the twins show

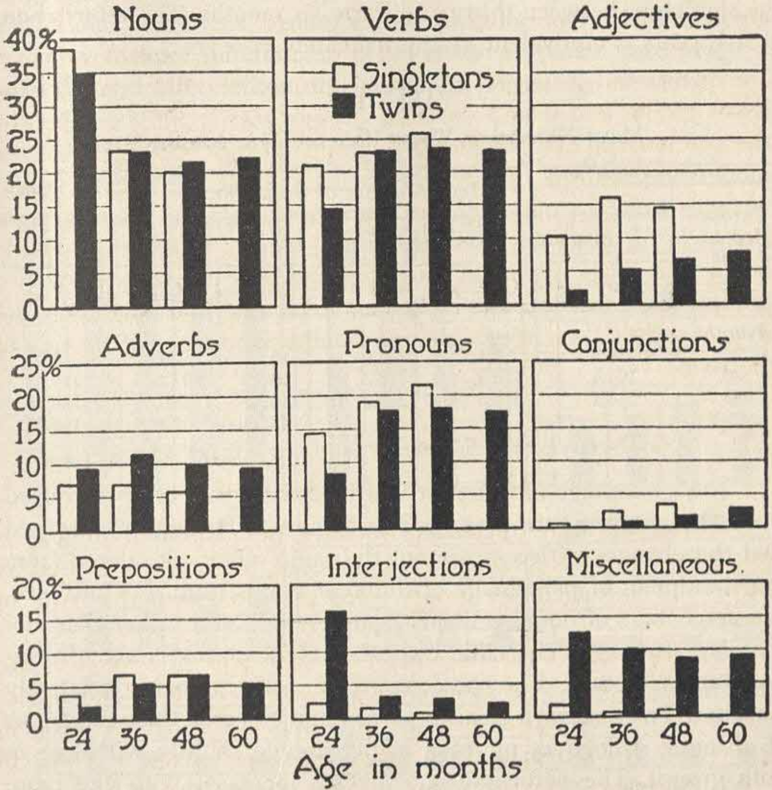

Figure 7. Percentage of parts of speech used by chronological age (based on total words used).

a very much greater proportion at all ages, especially at two years, than do singletons. This, no doubt, is related to the large proportion of emotionally toned responses which the twins showed in the functional analysis, and like it, is somewhat indicative of maintenance of infantile language. The miscellaneous parts of speech include words which were difficult to classify more positively, such as "uhm-uh," "uh-uhm" used so frequently for "yes" and "no." These appear to be used to a considerably greater extent by twins than by singletons. This then seems to show a tendency to attain adult standards more slowly.

Table III shows the mean number of total words used by both singletons and twins. This shows differences similar to those in the mean length of response. The singletons have but a slight advantage at two years, but show an increasing advantage with age. The 158 words attained by the twins at five years are equaled in number by the singletons between thirty and thirty-six months. The retardation at five years is equivalent to approximately two years.

TABLE III

Mean Number of Words Used by C. A. and Sex

Chronological Age	*Mean Number of Total Words*					
	Singletons (McCarthy)			*Twins*		
	Boy	Girl	All	Boy	Girl	All
months						
24	36.8	87.1	66.0	40.11	70.9	55.90
36	164.4	176.2	170.3	116.80	112.45	114.63
48	213.4	218.5	216.3	146.31	146.00	146.15
60				143.30	173.20	158.25

The percentage of different words of each part of speech used by each age group are presented in Figure 8. It may be observed that the changes with age are not the same when considered from the standpoint of percentage of different words used. For instance, the percentage of nouns remains relatively constant rather than decreasing with age. The child's vocabulary of nouns is actually increasing with age. The percentage of verbs remains relatively constant. Twins show a smaller percentage of verbs than singletons at all ages. Adjectives increase in percentage slightly with age in both groups. The percentage of adverbs increases with age more in the singleton group than in the twin group. Pronouns decrease in percentage with age although the actual number after three years remains about constant. Conjunctions increase in percentage a little in both groups and agree closely in total percentages. The percentage of prepositions decreases with age in twins and remains constant in singletons. Interjections decrease in percentage with age. The twins, however, show even at five years, a percentage almost equal to the two-year-old singleton. The miscellaneous words decrease with age and are higher in percentage for the twin group than for the singleton group.

The chief differences between singletons and twins in percentage of parts of speech used are in verbs, adjectives, pronouns, conjunctions, and interjections. The twins differ from the singletons in showing a smaller percentage of verbs at two years, a smaller percentage of adjectives, pronouns, and conjunctions at all ages, and a greater percentage of interjections at all ages. In the percentages of parts of speech of the different words, adjectives, pronouns, and conjunctions do not show great differences in proportion between twins and singletons. Adverbs, interjections, and verbs continue to show distinct differences. Twins show a greater percentage of adverbs and interjections and a smaller percentage of verbs.

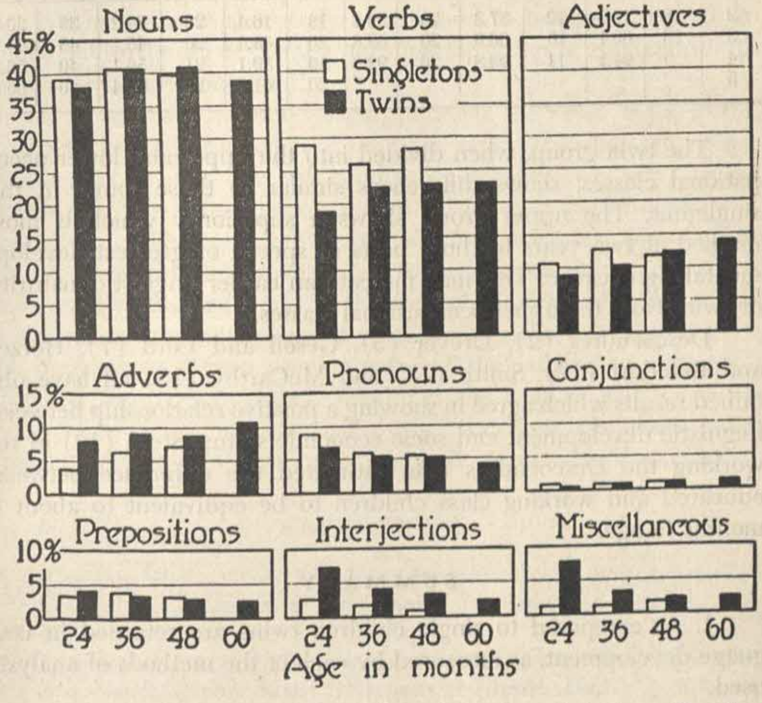

Figure 8. Percentage of parts of speech used by chronological age (based on different words used at each age group).

The sex differences in percentage of parts of speech, of the total words used, are of the same order as in the singleton group although they are less in magnitude in most instances. Girls are in advance. The greatest differences appear at two years, indicating, perhaps the earlier linguistic maturity of the girls.

The total number of words used also favors the girls. As in the mean length of responses, the boy and girl twins do not differ at three and four years although they show striking differences at two and five years. In the number of different words used (Table IV) girls are superior at every age except three years, when the boys exceed by a slight amount.

TABLE IV

MEAN NUMBER OF DIFFERENT WORDS USED BY TWINS AND SINGLETONS

Years	*Singletons (McCarthy)*						*Twins*					
	No cases	Boys	No cases	Girls	No cases	All	No cases	Boys	No cases	Girls	No cases	All
2	8	16.6	12	37.3	20	29.1	19	16.4	20	24.2	39	20.4
3	10	60.1	10	66.0	20	62.8	20	45.4	20	45.1	40	45.3
4	9	91.1	11	93.8	20	92.6	19	59.1	21	56.1	40	56.5
5							20	61.3	20	69.4	40	65.3

The twin group, when divided into the upper and lower occupational classes, shows differences similar to those found in the singletons. The upper group shows a superiority which is most marked at two years in those parts of speech of greatest developmental significance. This may indicate an earlier linguistic maturity of twins from the upper occupational classes.

Descoeudres (2), Drever (5), Gesell and Lord (7), Hetzer and Reindorf (12), Smith (18) and McCarthy (13) all have obtained results which agree in showing a positive relationship between linguistic development and socio-economic status. Stern (19) in reworking the Descoeudres data estimated the difference between educated and working class children to be equivalent to about 8 months in age.

SUMMARY

1. As compared to single children twins are retarded in language development, as measured by each of the methods of analysis used.

2. This language retardation increases with age, within the age period covered (two to five years) and is most clearly shown in the comparison of the findings of mean length of response.

3. Both in the analysis according to the structure of the sentence, and in the word analysis, twins show the greatest retardation in those phases which make the greatest change with age.

4. This is also true in the functional analysis with the exception of two phases. Egocentric responses and dramatic imitation, which are both affected, probably by the social advantage of the twin situation, show a superior development in twins as compared with singletons.

5. A small sex difference in favor of the girls appears in all methods of analysis. These sex differences are not as great as in the case of singletons, due possibly to the operation of the social factor.

6. Twins of the upper three occupational classes are superior in all methods of analysis to the twins of the lower three occupational classes.

7. Singletons of the lower three occupational classes are superior in mean length of response to the twins of the upper three occupational classes. They are frequently superior in the phases of the other analyses.

8. The twins in this study were found to be approximately 10 points below the IQ of the slightly superior group of singletons with whom they were compared.

If in the twin situation each child has the other for a model in speech a large proportion of the time, rather than adults or older children as in the case of singletons, perhaps we may expect slower progress with a poorer model. Nothing in the situation prevents two children from listening simultaneously to what adults say, although the time spent in practicing speech might be limited and considerably reduced. In the first place only one person can talk at a time and in the second place perhaps the activities or even the presence of a twin provide some of the satisfaction otherwise obtained from conversation. Whatever the factors may be however, the evidence is clear that during the preschool years twins progress toward adult use of language at a relatively slow rate.

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Five—The Origin of Names

JEAN PIAGET

REPRINTED FROM PAGES 63-67 AND 80-85 *The Child's Conception of the World.* LONDON: ROUTLEDGE AND KEGAN PAUL, LTD., 1929, BY PERMISSION OF THE PUBLISHER.

In a long series of books and monographs, Piaget, a Swiss child psychologist, has been interested in the child's ideas, his conception of the world about him, his language, and his manner of thinking. The following books by Piaget have appeared in English: THE LANGUAGE AND THOUGHT OF THE CHILD, JUDGMENT AND REASONING IN THE CHILD, THE CHILD'S CONCEPTION OF THE WORLD, THE CHILD'S CONCEPTION OF PHYSICAL CAUSALITY, and THE MORAL JUDGMENT OF THE CHILD. *The three excerpts from Piaget that are included in the present chapter are a very small sample of Piaget's works but may serve to introduce the student to him.*

His writings should be thought of as interpretations of the child based upon wide experience, brilliant insight, and pioneer investigations. His work is not statistical; the age standards that he suggests for certain stages are only approximations. Many of his theories will require revision in the light of further studies, but he has provided child psychology with a large number of stimulating observations and ideas.

The section immediately following is concerned with what Piaget has called "nominal realism," the child's tendency to treat names as if they were parts of the objects to which they refer rather than as arbitrary designations.

THE ORIGIN OF NAMES

During the first stage (five to six) children regard names as belonging to things and emanating from them. During the second stage (seven and eight) names were invented by the makers of the things—God or the first men. In the case of the first men, the child generally supposes that the men who gave the names are those who made the things: the sun, the clouds, and so on. During the third stage, which appears about the age of nine or ten the child regards names as due to men of no particular identity, while the name is no longer identified with the idea of creation.

The following are answers illustrating the first stage where the name emanates directly from the thing.

LAV (6-6) says that names are *"to call things by."* "How did name begin? How did the sun get its name?—*I don't know.*—Where did your name 'Jules' come from? Who gave it you?—*I don't know.*—Your father? —*Yes.*—And where did the name of the sun come from?—*The sky.*—Is it the sun or the name of the sun which comes from the sky?—*The sun.*—And where does its name come from?—*The sky.*—Did someone give the sun its name or did it get it by itself?—*Someone gave it.*—Who?—*The sky.*" "Where did the Arve get its name?—*From the mountain.*—Tell me, did people give it its name?—*No.*" And so on.

FERT (7) concerning the name of the Salève: "How did it get its name in the beginning?—*From a letter.*—And where did the letter come from?—*The name.*—And the name?—*From the mountain.*—How did the name come from the mountain?—*By a letter.*—Where did the letter come from?—*The mountain.*—Clouds are called clouds, aren't they? Where does the name of the clouds come from?—*The name? That is the name.*—Yes, but where does it come from?—*The clouds.*—What do you mean when you say it comes from the clouds?—*It's the name they've got.*—But how did the name happen? How did it begin?—*By itself.*—Yes, but where did the name come from?—*By itself.*"

These children evidently distinguish the name from the thing named, but can only conceive the name as coming from the thing itself. The following case is intermediate between this stage and the next.

STEI (5-6): "Have you a name?—*Yes, André.*—And that?—*A box.*—And that?—*A pen.*—What are names for?—*They are what you can see when you look at things* (Stei thus believes that one has only to look at a thing to 'see' its name)—Why have you got a name?—*So as to know what I'm called.*—Then what are names for?—*To know what things are called.*—How did the sun get its name in the beginning?—*I don't know.*—What do you think?—*Because the sun made the name, the sun gave it in the beginning and so the sun is called sun.*—And how did you get your name?—*We have to be christened.*—Who christened you?—*The clergyman.*—And did you take your name?—*The clergyman makes it for us.*—How did the moon get its name?—*The moon? The moon is called the moon.*—How did it start being called moon?—*God called it that in the beginning.*—How did the clouds start being called clouds?—*God started them by making them.*—But are the clouds' names the same thing as the clouds?—*Yes, the same thing.*—How did the Salève first get its name?—*By itself.*—Did the Salève give itself its name or did someone give it its name?—*It was always called Salève.*" Stei thus comes back to the idea that the name emanates from the thing.

During the second stage, this belief suggested in passing by Stei, becomes more and more pronounced; the name comes from the person who made the thing and is thus from the beginning intimately connected with the thing itself. The following examples illustrate this.

FRAN (9): "You know what a name is?—*It's to know what the children are called.*—Where do names come from? How did they begin?— *Because God said, 'Now it's time to make children and then they must be called by names.'*—What does that mean to 'be called by names'?—*So as to know which children.*—How did the table get its name in the beginning? —*God said, 'Tables must be made to eat from and people must know what they are for.'*"

BAB (8-11): "How did the sun get its name in the beginning?—*It was called that.*—Who by?—*People.*—What people?—*The first men.*"

All the answers are similar. For most of the children the sun, the sky, the mountains, the rivers, and so on were all made by the first men, but as this question is to be studied later it need not concern us here.

Finally, during the third stage, names were not given by the makers of the things but by other men "savants," and the like.

CAUD (9-6): "*The sun was first called 'sun' by a man and afterwards everybody knew*—Who was the man?—*A learned man (un savant).*—What is a 'savant'?—*A man who knows everything.*—What did he do to find out the names? What would you do if your were a 'savant'?—*I should try and think of a name.*—How?—*In my head.*" Caud then went on to say that God made the sun, fire, and so on, and that their names were given them by "savants."

The evolution of the answers thus seems to show a gradual decrease in nominal realism. During the first stage the name is in the thing. During the second it comes from men but was made with the thing. It is thus still, so to speak, consubstantial with the thing and may possibly still be regarded as situated in the thing. During the third stage the name is at last regarded as due to the person who thinks about the thing.

The study of answers to another question entirely confirms these views. This question consists in asking whether things have always had their names or whether they existed before they had names. This question it will be seen serves principally as a confirmatory proof for the previous question. The two questions should therefore not be set immediately after one another or the child will simply draw his conclusions from what he has just said without considering the new problem.

In the great majority of cases the answers to the two questions were in perfect accord, that is to say children of the first and second stages maintained that things did not exist before having names, while the opposite was held by children of the third stage.

The following examples are of children who regard things as having always had names.

Zwa (9-6): "Which were first, things or names?—*Things.*—Was the sun there before it had its name?—*No.*—Why not?—*Because they didn't know what name to give it* (would not have known; but the use of the conditional is difficult for children).—But before God gave it its name was there a sun?—*No, because he wouldn't know where to make it come from.* (The idea of nonexistence always causes difficulty).—But it was there already?—*No.*—And were there clouds before they had names?—*No, because there wasn't anyone in the world* (!)" We then tried a question outside the scheme, but naturally suggested by Zwa's metaphysics: "If a thing wasn't there could it have a name?—*No.*—Long ago men used to believe there was a certain fish in the sea which they called a 'chimera' but there wasn't really any such fish . . . so can't a thing that doesn't exist have a name?—*No, because when God saw that the things didn't exist he wouldn't have given them names.*—Have fairies got a name?—*Yes.*—Then there are things that don't exist and have a name?—*Only fairies.*—Why are there things that don't exist and yet have a name?—*God made up other names and they don't exist.*"

This inability to dissociate names from things is very curious. The following observation, involving the same idea, we owe to a colleague, Dr. Naville. A little girl of 9 asked: "Daddy, is there really God?" The father answered that it wasn't very certain, to which the child retorted: "There must be really, because he has a name!"

Mart (8-10): "Has the sun always had its name?—*Yes, it always had its name when it was born.*—How was the sun born?—*Like us.*" Same answer for the clouds, the Salève, and so on.

Pat (10): "Before the sun had its name was it already there?—*Yes.*—What was it called?—*The sun.*—Yes, but before it was called sun was it there?—*No.*"

Bab (8-11) whose answers have already been quoted: "Has the sun always had its name or was there a sun before it had a name?—*It's always had its name.*—Who gave it its name?—*People* (*des Messieurs*).—And before people gave it its name was it there?—*Yes.*—What was it called?—*Sun.*—Who gave it its name?—*People.*"

The following examples are of children who have come to regard things as existing before they had names. These children are nine or ten years old and almost all belong to the third stage as previously distinguished.

> MEY (10): "Tell me, did the sun exist before it had a name?—*Yes, men gave it its name.*—And were there clouds before they had names?—*Of course.*"
>
> VEIL (9-6): "Did the sun exist before it had a name?—*It was already there.*—What was it called then?—*It hadn't yet got a name.*"

There are children who consider that only animals and plants or animals alone, know their names. Even children of advanced intelligence, like Mey, will maintain that perhaps trees know their names.

> MEY (10): "Does a dog know its name?—*Yes.*—A fish?—*Yes, because if we know we belong to the world* (that is, that we are men) *fish ought to know it too.*—And does the sun know it is called the sun?—*No.*—Why not?—*Because it isn't alive.*—Does the wind know its name?—*No.*—Do the trees know that is what they are called?—*No, because we couldn't make them know it.*—Why not?—*They wouldn't understand.*—Then they don't know their names?—*Perhaps they may, perhaps not.*—Why 'perhaps not'? —*Trees can't learn things.*—And why 'perhaps they may'?—*They see other trees besides themselves and think they are the same thing.*—And what does that do?—*They know they are oaks but they can't see it.*"

Finally, there are children who refuse a knowledge of names to everything. The average age of this group was nine to ten. The children who associated knowledge of name with movement (like Mart) had an average age of seven. This evolution agrees closely with what will be found later in the study of children's animism.

THE INTRINSIC VALUE OF NAMES

So far we have studied names under what might be called their ontological aspect, that is, their existence, place, and origin. There remains the logical aspect: Are names merely signs or have they an intrinsic logical value? The two problems are strictly dependent on one another and it is evident that names so far as they are situated in things must be regarded as absolute. But though the ontological realism and the logical realism of names may have the same roots, their persistence may perhaps differ. This—that logical realism lasts much longer than ontological—is precisely what we shall hope to show. Even those children who localize the name in the head and who believe in the recent origin of names, continue to hold that names imply not the thing but the idea of the thing; for example, the sun is called thus because it is bright and round and so on.

To begin with the question: Could names be changed? Two stages were distinguished. Before the age of ten the children said not, after an average age of ten they agreed that they could. Between the two were several intermediate cases. The following examples are of the first stage.

FERT (7): "Are you called Albert?—*Yes.*—Could you have been called Henry? Would it have been just the same?—*No.*—Could the Salève have been called 'Jura' and the Jura 'Salève'?—*No.*—Why not?—*Because they are not the same thing.*—And could the moon have been called 'sun' and the sun 'moon'?—*No.*—Why not?—*Because the sun makes it warm and the moon gives light.*"

ROC (6-6) admits that God might have changed the names: "Would they have been right then or wrong?—*Wrong.*—Why?—*Because the moon must be the moon and not the sun and the sun must be the sun!*"

FRAN (9): "Could the sun have been given another name?—*No.*—Why not?—*Because it's nothing else but the sun, it couldn't have another name.*"

ZWA (9-6) knows some German and might have been expected to understand the relative nature of names. But he did not: "Could names be changed and things given other names? You are called 'Louis,' could you have been called 'Charles'?—*Yes.*—Could this chair have been called 'Stuhl'?—*Yes, because it's a German word.*—Why are there other names in German? Why don't they talk like we do?—*Because they can speak a different way.*—Have things got more than one name?—*Yes.*—Who gave them the German names?—*God and the Germans.*—You say names could be changed. Could the sun have been called 'moon' and the moon 'sun'?—*No.*—Why not?—*Because the sun shines brighter than the moon.*—Have you a brother?—*Gilbert.*—Could Gilbert have been called 'Jules'?—*Yes.*—Well, couldn't the sun have been called 'moon'?—*No.*—Why not?—*Because the sun can't change, it can't become smaller.*—But if every one had called the sun 'moon,' and the moon 'sun,' would we have known it was wrong? —*Yes, because the sun is always bigger, it always stays like it is and so does the moon.*—Yes, but the sun isn't changed, only its name. Could it have been called . . . etc.?—*No.*—How would one know it was wrong?—*Because the moon rises in the evening, and the sun in the day.*"

BUS (10) says that nothing could be changed: "*because they wanted to give the sun the name of sun.*—If in the beginning the first men had given different names, would we have seen by now that they were wrong or would we never have known?—*We should have seen.*—How?—*Because the sun is hot and the moon is not hot.*"

The following is an intermediate case in which names might have been changed but "it wouldn't have been so good."

DUP (7-6, a girl, very forward): "Could the sun have been called 'stool'?—*Yes.*—No one would have noticed anything?—*No.*—Could the table have been called 'chair'?—*Yes, no.*—Could it or not?—*Yes, it could.*" A star was called a "star" "*because people thought that name would go best.*

—Why?—*I don't know.*—Could it have been called 'nail'?—*It wouldn't have been so good.*"

Dup shows a great advance on the preceding subjects in having partly realized the conventional character of names and above all in having understood that if names had been different no one would have known. None the less she seems to believe in a certain harmony between the name and the idea of the thing without venturing definitely to state its nature.

The following examples of the second stage show children who realize that the character of names is not entirely arbitrary—that is a later stage—but conventional.

MEY (10): "Could you have been called Henry?—*Yes.*—Could the Jura have been called 'Salève' and the Salève 'Jura'?—*Yes, because men could have changed names or made them the opposite.*—Could the sun have been called 'moon'?—*Why not?*—Could it have? Could that (a table) have been called a chair and that (a chair) a table?—*Yes.*—If the sun had been called 'moon,' would we have known it was wrong?—*No.*—Why not? —*We couldn't have known it was wrong.*—Why not?—*Because they would have given the name 'moon' to the sun. They wouldn't have seen any difference.*"

BAB (8-11) after having given a number of primitive answers suddenly realized his sophistry and replied to the last question quite correctly: "Could the Salève have been called 'Jura' and the Jura 'Salève'?—*Yes.*— Why?—*Because it's the same thing.*—Could the sun have been called 'moon' and the moon 'sun'?—*Yes.*—Should we have known the names were changed?—*Yes.*—Why?—*Because we'd have been told.*—If no one had told us should we have known?—*No.*—Why not?—*Because names aren't marked on things.*"

Thus at about the age of nine or ten, that is to say just at the age when all the preceding questions were solved, the child admits that names could have been changed and that no one would have known. But this answer does not alone prove that the name has no intrinsic value. It simply proves the decline of ontological realism: Names are no longer tied up to the things they represent.

Indeed, the question, "Why does a particular object have a particular name?" is not solved until after the preceding question, and it is in fact the hardest of all the questions.

The question just stated gives rise to the following stages: Until the age of ten, all names contain the idea of the thing. During the second stage (ten and eleven) there is simply some sort of harmony between the name and the idea; the name "fits," goes well and so on. That is to say the idea of the thing is still present in some measure, but other names containing the same idea might have been chosen. Finally, after eleven or twelve the name contains in itself nothing. It is purely a sign.

The following are examples of the first stage:

Horn (5-3): "Why is the sun called what it is?—*Because it behaves as if it was the sun.*"

Roc (6): "Why was the sun given that name?—*Because it shines.*—And the Salève?—*Because it is a mountain.*—Why are mountains called 'mountains'?—*Because they are all white.*"

Bab (8-11): "Why is the sun called what it is?—*Because it is all red.*—Why is the moon called what it is?—*Because it is all yellow.*—And the Salève?—*Because it is called the Salève.*—Why?—*Because . . .* —For a reason or for no reason?—*For a reason.*—Why?—. . .—Why are clouds called like that?—*Because they are all gray.*—Does 'clouds' mean that they are all gray?—*Yes.*"

Veil (9-6) believes the sun is so called "*because it heats*"; a table "*because it is used for writing,*" and so on.

Bus (10): The Salève is so called "*because it rises up*"; the stars "*because they are that shape*"; a stick "*because it is thick.*" "Does the stick mean that it is thick?—*It is long.*"

Fran (9): The Salève is called "Salève" "*because it is a mountain which slopes on all sides.*"

These examples might be multiplied indefinitely. The principle is the same in all: A word is always associated with its context until it comes to be regarded as implying the whole context.

In the second stage may be grouped those children who, while not so boldly affirming the connection between names and their content yet feel that there is none the less a harmony.

Dup (7-6, a girl): "Why are the stars called 'stars'?—*Because people thought it the best name.*—Why?—*I don't know.*—(see Dup's earlier answers.) The sun was given the name 'sun', "*because the sun gives more light* (than the moon) *and I think too that the name of sun goes best for the sun, because the people who gave it that name thought it suited it best.*"

Mey (10) after having solved one question said, however, that the sun was so called: "*because people thought it was a good name and a bright one.*"

Dup and Mey do not say that the name of the sun implies light. They merely say there should be a connection. In principle this is true, but what in fact they maintain is not the result of a historical hypothesis but is simply the last traces of nominal realism.

Among the children who solved the question, Mey was the only one we have so far found who succeeded before the age of eleven or twelve, and he only arrived at the solution at the end of the examination and after first giving the answers quoted above.

Mey (10) . . . "Why is the moon called by that name?—*Just, because it is, for no reason.*—Why is the Salève so called?—*It's a name people found for it.*—Could it have been called 'Nitchevo'?—*Certainly, because that's a name too.*"

GEN (11): "Why is the sun called what it is?—*Not for any reason, it's just a name.*—And the moon?—*No reason. Anything can be called by any name you like.*"

It is thus not until this question is solved that the child can be supposed to have understood the arbitrary nature of names. Nominal realism in its ontological form is discarded after the age of nine or ten, but the realism of the logical form does not start to disappear before eleven or twelve. In short, logical realism arises from ontological realism but lasts longer.

Six—The Idea of Immanent Justice

JEAN PIAGET

REPRINTED FROM PAGES 250-253 *The Moral Judgment of the Child.* LONDON: ROUTLEDGE AND KEGAN PAUL, LTD., 1932, BY PERMISSION OF THE PUBLISHER.

According to Piaget, the child at an early age sees punishment, retribution, and justice as inherent in the external world. He does not distinguish clearly between consequences that arise from the physical universe and the consequences arising from the social world of man.

During the early years of his life, the child affirms the existence of automatic punishments which emanate from things themselves, while later, under the influence of circumstances which affect his moral growth, he probably abandons this belief. This is what we shall now endeavor to show.

We told the children three stories in this connection.

Story I. Once there were two children who were stealing apples in an orchard. Suddenly a policeman comes along and the two children run away. One of them is caught. The other one, going home by a roundabout way, crosses a river on a rotten bridge and falls into the water. Now what do you think? If he had not stolen the apples and had crossed the river on that rotten bridge all the same, would he also have fallen into the water?

Story II. In a class of very little children the teacher had forbidden them to sharpen their pencils themselves. Once, when the teacher had her back turned, a little boy took the knife and was going to sharpen his pencil. But he cut his finger. If the teacher had allowed him to sharpen his pencil, would he have cut himself just the same?

Story III. There was a little boy who disobeyed his mother. He took the scissors one day when he had been told not to. But he put them back in their place before his mother came home, and she never noticed anything. The next day he went for a walk and crossed a stream on a little bridge. But the plank was rotten. It gave way, and in he falls with a splash. Why did he fall into the water? (And if he had not disobeyed would he have fallen in just the same?)

The first two questions were put by Mlle Rambert to 167 children from Geneva and the Vaudois Jura. We ourselves put question III and other analogous questions to children from Neuchâtel. With regard to the first two, Mlle Rambert was able to obtain statistics showing very clearly the influence of the child's mental age. Leaving aside the uncertain reactions which constitute about one-fifth of the total, the answers affirming the existence of immanent justice, those, that is to say, where the subject maintained that if the child had not stolen or disobeyed he would not have fallen into the water or would not have cut himself, revealed the following percentages:

Age 6	Ages 7 to 8	Ages 9 to 10	Ages 11 to 12
86%	73%	54%	34%

In addition to this it should be noted that in a class of backward children of thirteen to fourteen the proportion of answers of the same type was found to be 57%, which shows again that these answers are in inverse proportion to the mental age. Here are examples of this belief in the immanent justice of things.

Dep (6), Story I: "What do you think of the story?—*It serves him right. He shouldn't have stolen. It serves him right.*—If he had not stolen the apples, would he have fallen into the water?—*No.*"

Chr (6), Story III: "Why did he fall in?—*God made him, because he had touched the scissors.*—And if he hadn't done what was wrong?—*Then the board would have held out.*—Why?—*Because he didn't touch* (would not have touched) *the scissors.*"

Sa (6), Story I: "What do you think of that?—*The one who got caught was sent to prison, the other was drowned.*—Was that fair?—*Yes.*—Why?—*Because he had been disobedient.*—If he hadn't been disobedient would he have fallen into the water?—*No, because he hadn't* (= would not have) *been disobedient.*"

Jean (6), Story II: He cut himself "*because it was forbidden to touch the knife.*—And if it had not be forbidden, would he also have cut himself?—*No, because the mistress would have allowed it.*"

Gra (6): Same answers for Story I. "What happened?—*The bridge cracked.—Why?—Because he had eaten the apples.*—If he had not eaten the apples, would he have fallen into the water?—*No.*—Why?—*Because the bridge would not have cracked.*"

Pail (7), Story I: "What do you think of that?—*It's fair. It serves him right.*—Why?—*Because he should not have stolen.*—If he had not stolen, would he have fallen into the water?—*No.*—Why?—*Because he would not have done wrong.*—Why did he fall in?—*To punish him.*"

Sca (7): "What do you think about it?—*Oh yes, I know. If we do* *anything, God punishes us.*—Who told you that?—*Some children. I don't know if it's true.*" Story II: "*It serves him right. You ought to obey teacher.*—And if the teacher had allowed it, would he have cut himself sharpening a pencil?—*No. He wouldn't have cut himself if the teacher had allowed it.*"

Boe (8), Story III: "What do you think?—*It serves him right. You shouldn't disobey.*—And if the teacher had allowed it, would he have cut himself sharpening the pencil?—*No, he wouldn't have been cut, because he wouldn't have done anything.*"

Pres (9), Story I: "What do you think?—*He was punished as much as the other one, and even more.*—And if he had not stolen the apples, would he have fallen into the water as he was crossing the river?—*No, because he would not have needed to be punished.*"

The (10), Story I: "*He was punished. Neither of them ought to have stolen. If he hadn't fallen into the water he would have been caught.*—And if he hadn't been caught?—*He'd have fallen into the water. Otherwise he'd have gone on stealing.*"

Dis (11), Story I: "*He had his punishment too.*—Was it fair?—*Yes.*—And if he hadn't stolen the apples would he have fallen into the water?—*No, because* (in that case) *he didn't have to be punished.*"

Here are some examples of children who no longer believe in immanent justice, at least not in the stories we told them. But this does not prevent this belief from attaching itself to other objects, by becoming displaced and gradually spiritualized.

Gros (9), Story III: "Why did he fall in?—*Because the plank was worn out.*—Was it because he had disobeyed?—*No.*—If he had not disobeyed would he also have fallen in?—*Yes. He'd have fallen in just the same. The plank was worn out.*"

Fleu (12), Story I: "And if he hadn't stolen the apples would he have fallen in too?—(He laughs). *The bridge isn't supposed to know whether he has stolen the apples.*"

Bar (13): "*It was perhaps a coincidence. But the punishment was what he deserved.*"

Fran (13): Story I: "And if he had not stolen the apples, would he have fallen into the water?—*Yes. If the bridge was going to give way, it would have given way just the same, since it was in bad repair.*"

But between two groups of clear cases, we find a series of intermediate examples, which are very interesting from the point of view of child logic, and whose original feature consists in saying that the event mentioned in our stories is certainly a punishment, but that it would have happened in any case, even if there had been no previous offence.

Seven—Communication Between Children

JEAN PIAGET

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Communication between two adults is frequently subject to error, but Piaget indicates that communication between young children is even more defective unless the ideas to be communicated are already well-known to both children.

The matter can be introduced as an amusement or a competition: "Are you good at telling stories? Very well then, we'll send your little friend out of the room, and while he is standing outside the door, we'll tell you a story. You must listen very carefully. When you have listened to it all, we'll make your friend come back, and then you will tell him the same story. We shall see which of you is best at telling stories. You understand? You must listen well, and then tell the same thing . . ." and so on. Repeat the instructions as often as necessary, and stress the need for a faithful rendering.

Then one of the subjects is sent out of the room, and the set piece is read to the other. The more complicated passages are repeated, everything is done to make the subject listen, but the text is not altered. Then one or the other of the following methods is adopted: (They have been used alternately, the one serving as a test of the other.) Either the child who has been waiting outside in the passage, and whom we shall call the *reproducer*, is sent for, and everything that the other child (whom we shall call the *explainer*) says to him is taken down in *extenso*; or else the explainer is asked to tell us a story in the first instance, and is then sent to tell the same story to the reproducer out in the lobby or in the garden, that is, in our absence, and with the injunction to take as much time as he likes. In both cases the story, as told by the reproducer, is taken down verbatim.

When the experiment is over, the two children exchange parts; the explainer is sent out of the room and becomes the reproducer in this second test, a new story is told to the former reproducer who now becomes explainer, and everything is done as in the previous case.

We have carried out with these methods some hundred experiments on 30 children from seven to eight, taken in pairs (that is, 15 couples with 4 experiments per couple, say 2 explanations and 2 stories), and on 20 children from six to seven (10 couples with 4 experiments per couple).

Here are the stories which were used:

Story I. Epaminondas is a little Negro boy and he lives in a country where it is very hot. His mother once said to him: "Go and take this shortbread cake to your granny, but don't break it." Epaminondas put the shortbread under his arm, and when he got to his grandmother's the shortbread was in crumbs. His granny gave him a pat of butter to take back to his mother. This time Epaminondas thought to himself: "I shall be very careful." And he put the pat of butter on his head. The sun was shining hard, and when he got home the butter had all melted. "You are a silly," said his mother, "you should have put the butter in a leaf, then it would have arrived whole."

Story II. Once upon a time, there was a lady who was called Niobe, and who had 12 sons and 12 daughters. She met a fairy who had only one son and no daughter. Then the lady laughed at the fairy because the fairy only had one boy. Then the fairy was very angry and fastened the lady to a rock. The lady cried for ten years. In the end she turned into a rock, and her tears made a stream which still runs today.

Story III. Once upon a time, there was a castle, and in it were a king and queen who had three sons and one daughter. Near the castle was a wicked fairy who did not like children. She took the king and queen's children to the seashore, and changed them into four beautiful white swans. As their children had not come home, the king and queen went to look for them everywhere, and they came right down to the seashore. There they saw four beautiful swans, who told them that they were their children. The swans stayed on the sea for a very long time, and then they went away to a very cold country. After many years they came back to where their castle was. There was no castle there any longer, and their parents were dead. The swans went into a church and they were changed into three little old men and one little old woman.

So far, then, the method is quite simple. You read one of the stories to the explainer, but you must not appear to be reading, and you must talk in the most natural manner possible. The explainer then tells the story to the reproducer, who finally serves it up to you again.

But this is not all. Once the reproducer's story has been obtained and taken down in its entirety, the explainer is taken aside for a few moments, and the reproducer is asked a certain number of questions on the points that have been omitted, so as to ascertain whether he has really failed to understand them. He may either have forgotten them or he may not know how to express them. In order to judge of the child's degree of understanding these factors must at all costs be eliminated, so as to clear the ground for a more searching investigation. If in the story of Niobe, for example, the end is forgotten, the child is asked whether there is nothing about a stream. Thus by means of questions, vague at first and then more and more precise, and with the help of that division into points which we shall give in the next paragraph, the reproducer's degree of understanding can be properly put to the test. When this has been done, the explainer is questioned in the same manner, to see whether he has really understood the points which appear doubtful.

We have divided each of our set pieces into a certain number of points, as is done in the sifting of evidence, so as to see which of these points have been reproduced or omitted by the subjects, and instead of choosing a large number all bearing on questions of detail, we have restricted ourselves to a small number of rubrics connected solely with the understanding of the story. In estimating the correctness of each point, moreover, we have taken no notice in parceling out the material of the factors that were not essential to the understanding of the story. Thus in the tale of Niobe the name of Niobe plays no part whatsover; it is sufficient if mention is made of "a lady" or even "a fairy." Similarly, "12 boys and 12 girls" can be changed into "many children" or "3 children," and the like, provided a difference is made between the number of children belonging to "the lady" and that of those belonging to the fairy.

Here, in detail, are the points taken into consideration in story II:

(1) Once there was a lady (or a fairy, and so on). (2) She had children (provided they outnumber those of the other fairy). (3) She met a fairy (or a girl). (4) This fairy had few children (or none at all, provided their number is inferior to the first lot). (5) The lady laughed at the fairy. (6) Because the fairy had so few

children. (7) The fairy was angry. (8) The fairy fastened the lady (to a rock, a tree, to the shore). (9) The lady cried. (10) She turned into a rock. (11) Her tears made a stream. (12) Which flows to this day.

It is obvious that each of these points except point (7), which can easily be taken for granted, and points (9) and (12) which are supplementary to the body of the story, are necessary to the comprehension of the story. It will be seen, moreover, that we are very generous in our estimates, since any alternation of detail is tolerated.

The stories of Epaminondas and of the four swans were parceled out according to exactly the same principles.

[Piaget here presents numerical data to show that in general a child does not understand another child's account at all well, whereas the original stories were quite well comprehended.]

Conversation between children is therefore not sufficient at first to take the speakers out of their egocentrism, because each child, whether he is trying to explain his own thoughts or to understand those of others, is shut up in his own point of view. This phenomenon occurs, it is true, among adults. But these have had at least some practice in argument or conversation, and they know their faults. They make an effort to understand and be understood, unless indeed distrust or anger reduces them to a childish state, because experience has shown them the appalling density of the human mind. Children have no suspicion of all this.

If children fail to understand one another, it is because they think that they do understand one another. The explainer believes from the start that the reproducer will grasp everything, will almost know beforehand all that should be known, and will interpret every subtlety. Children are perpetually surrounded by adults who not only know much more than they do, but who also do everything in their power to understand them, who even anticipate their thoughts and their desires. Children, therefore, whether they work or not, whether they express wishes or feel guilty, are perpetually under the impression that people can read their thoughts, and in extreme cases, can steal their thoughts away. It is obviously owing to this mentality that children do not take the trouble to express themselves clearly, do not even take the trouble to talk, convinced as they are that the other person knows as much or more than they do, and that he will immediately understand what is the matter.

These habits of thought account, in the first place, for the remarkable lack of precision in childish style. Pronouns, personal and demonstrative adjectives, "he, she" or "that, the, him," and so on, are used right and left, without any indication of what they refer to. The other person is supposed to understand. Here is an example:

> Gio (8) tells the story of Niobe in the role of explainer: "Once upon a time there was a lady who had 12 boys and 12 girls, and then a fairy a boy and a girl. And then Niobe wanted to have some more sons (than the fairy. Gio means by this that Niobe competed with the fairy, as was told in the text. But it will be seen how elliptical is his way of expressing it.) Then she (who?) was angry. She (who?) fastened her (whom?) to a stone. He (who?) turned into a rock, and then his tears (whose?) made a stream which is still running today."

From this account it looks as though Gio had understood nothing. As a matter of fact he had grasped nearly everything, and his understanding in relation to us was high. He knew for instance that the fairy was angry "because she (N.) wanted to have more children than the fairy." The pronouns distributed at random are therefore a characteristic of the style, and not a proof of lack of understanding. Gio knows perfectly well that it was the fairy who fastened N. to the rock and not *vice versa.*

It is easy to foresee the results of such a style. The reproducer, Ri (8), begins by taking N. for the fairy, and by thinking that it is N. who fastens the lady. After being put right on this point, he reproduces the story as follows:

> There was a lady once, she had 12 boys and 12 girls. She goes for a walk and she meets a fairy who had a boy and a girl and who didn't want to have 12 children. 12 and 12 make 24. She didn't want to have 24 children. She fastened N. to a stone, she became a rock, and so on.

One—A Critique of Experiments on Raising the IQ

FLORENCE GOODENOUGH

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On more than one occasion investigators have felt that a way has been found to produce marked increases in the child's IQ. Thus far these claims, on careful investigation, have been found to be exaggerated. Although the possibility of finding means of increasing human abilities should not be ignored, all claims that such means have been found should be subjected to close scrutiny.

The following discussion by Goodenough was stimulated by reports that attendance at nursery school caused large increases in intelligence test performance. It is now generally admitted that the average effect of nursery school upon the child's score on early intelligence tests is only a few points. Goodenough's discussion, however, is relevant to evaluating other claims that may well arise in the future.

Today as never before in history, the findings of the scientific laboratory play a vital part in our daily life. From the time of our morning orange juice with its carefully reckoned provision of vitamins and phosphorus to our nightly rest on some "scientifically tested" brand of mattress, the moments are few in which we are not being reminded, directly or indirectly, of the marvels of a scientific age. With the radio, the airplane, the automobile, television, and a thousand other constant evidences that the barriers of the impossible are steadily receding, it is hardly to be wondered at that when the lively imagination of Orson Welles conjured up the idea of a Martian invasion, his radio audience, whose minds had already been keyed to believe the unbelievable, found the hoax no more difficult to accept than other wonders of a miraculous age.

Not only in the field of mechanical invention but in the direct products of nature is science revolutionizing the world. The old saying about making two blades of grass grow where but one grew before has been more than realized. Not only have the two blades been grown but they have grown bigger and juicier. Thanks to scientific breeding and better care, the modern poultryman is able to average almost twice as many eggs per hen-day as his grandfather did; the dairyman has likewise increased the average yield of his cows, and the sheepgrower has improved the length and quality of wool.

In the realm of human behavior something has likewise been accomplished, though here we have less reason to point with pride. Yet there is little doubt but that scientific study of the learning processes, of work and fatigue, of individual differences in ability, interests, achievement, and skills, as well as of many other similar topics has slowly but gradually enabled us to improve our educational methods and to take up some of the lag between social and industrial progress.

Whatever may be the question as to its value, it is certain that the volume of research that is being carried on in such fields as psychology, human biology, sociology, education, and other sciences that deal directly with the behavior of the human animal is so great as to tax the powers of even the most rapid and indefatigable reader who makes an honest effort to "keep up with the literature." The inevitable result is that most of us are likely to form the habit of turning with increasing frequency to such short cuts as are provided by the various "digests," "abstracts," and the "summary and conclusions" that are to be found at the end of the greater number of the published accounts of research. Although for many of us this habit seems necessitated by the pressure of time, its hazards are obvious. The vagaries of human behavior are so many and so unexpected, and the difficulties of maintaining adequate scientific control of all variables that may play a part in producing a given result are so great that even the careful investigator may easily fall into the error of overlooking crucial points that, if properly considered, might lead to conclusions very different from those set down at the end of his article. Particularly is this likely to be the case when the study in question has to do with some matter on which its author has already expressed himself in unequivocal terms. For no matter how scrupulous a scientist may be, he is still likely to find it more pleasant to maintain himself in the right than to admit himself to have been mistaken. When therefore, certain parts of his data seem to fall in line with his theory, there is inevitably a strong temptation to regard these as the really crucial facts and to accept them as valid without further question. At the same time he is likely to seek for such explanations or qualifications of any facts that seem contradictory as will either minimize their importance or change their significance. If he succeeds in demonstrating to his own satisfaction that the discrepancies in question are in truth of little consequence as far as the major purpose of the research is concerned, the chances are that in summarizing his findings for the busy reader he may omit all mention of them, or at most pass them by with a brief mention.

The many and unexpected facts that have turned up in recent and well-controlled experiments in human behavior have rendered many unsophisticated readers well-nigh as credulous as were those who fled from Orson Welles' Martians, and even the best informed may easily be misled when a scientist of recognized standing "summarizes" only the positive findings of a piece of research and "concludes" from this incomplete presentation of the facts that certain principles are true. An outstanding example of this is to be seen in the widespread popular acclaim that has been given to certain studies that have recently appeared on the improvement of children's intelligence. In these reports, the claim is put forth that by proper training, especially such as is provided in the modern nursery school, the (true) intelligence of children can be raised to an astounding degree. Feeble-minded children, it is said, can be made normal and in proof thereof they cite an experiment in which every member of a group of 13 "feeble-minded" infants was brought within the range of normal mentality through training by moron nuresmaids in an institution for the feeble-minded! The children of feeble-minded women, if reared in superior homes, will, we are told, be indistinguishable in mentality from those of college professors. On the other hand we are warned that in a nonstimulating environment, children originally normal are likely to become truly feeble-minded.

In spite of my high personal regard for the investigators, I believe that their data have been grossly misinterpreted, particularly in the popular accounts intended for the untrained reader. The discrepancies between data and conclusions are in many instances marked. In other instances, essential features of experimental technique were so poorly controlled that no real conclusions seem justified. Nevertheless, the trustful reader who does not take time to examine the original reports with much care is almost certain to feel that the authors' claims have been amply substantiated. Particularly is this likely to be true of those who do not have immediate access to the main publications and who therefore rely upon the rather sensational articles in the popular magazines, or upon the abbreviated accounts of findings that have appeared in many of the educational journals and similar publications.

A large share of the results can be traced directly to two basic hypotheses, neither of which is accepted by most reputable scientists of the present day. The first hypothesis is that intelligence tests given to infants and to children of two or three years of age have essentially the same predictive value for later mental development as those given after school age. The second is that the reliability of such tests for the measurement of mental status at the time they are taken is so nearly perfect that the effect of statistical regression due to errors of measurement may be safely ignored. It is true that neither of these hypotheses has been formally stated as such in any of the publications. Nevertheless, both are implicit in their methods of handling the data and in the conclusions drawn therefrom, since if the hypotheses are untenable, the former become unsound and the latter unwarranted.

That intelligence tests given to infants provide no inkling as to their future mental status has been shown by Furfey and Muehlenbein (3), Bayley (2), and a number of others. Since these studies are based upon children reared in their own homes, it is impossible to impute the absence of relationship between earlier and later status to a major change in environmental stimulation. Honzik (4) carried the study of the mental development of the children in Bayley's group into the later ages of childhood and showed that while tests given after the age of about eighteen months provide some indication as to what the later course of development is likely to be, the error of estimate is still so large that a judgment of the future potentialities of an individual child on the basis of tests made before the age of six or seven years would frequently lead to gross error. Anderson (1) has made a very complete survey of the correlations between tests given at early and later ages from which he has drawn certain theoretical conclusions that are of major significance for all students of the principles of mental growth.

Not only do tests given early in life have little value for prognosis, but even when the results are looked upon as measures of present status only, inaccuracies are frequent. This is particularly true of the first measurement taken, when the situation, and often the examiner as well, may be unfamiliar to the child. Such factors as shyness, negativism, and the like that interfere with his making as good a showing as he should are likely to be at a premium at the time of his first visit to the laboratory. On later testings, better cooperation is usually secured, and his IQ will show a corresponding increase. Usually, however, this gain will not occur in all the cases of a given group. As a result of lucky guesses on marginal items, more lenient scoring on the part of a particular examiner, and so on, the initial scores of some children may actually exceed those earned on a later occasion.

We may say, then, that a child's test score on any single occasion may be represented by the following formula:

$$S_{obt} = S_t \pm E_1$$

where S_{obt} = the score obtained
S_t = the score corresponding to his true ability at the time of measurement
and E_1 = the error of measurement which may, of course, be either negative or positive in sign.

If, then, a group of children of varying initial IQ is separated into smaller groups, putting those with the highest initial scores in one classification, those who stood next highest in another, and so on down to the lowest, it follows as a mathematical necessity that not only that portion of the score which is due to the child's true level of ability but also that due to chance will play a part in determining the grouping, and since errors of measurement are uncorrelated with each other, it will also follow that when these children are retested, be it on the following day or after an interval of years, those originally testing highest will appear to lose while those originally testing low will appear to gain. This tendency is known as the "regressive shift."

In practically all the studies from the cited laboratory study, the practice just described has been followed. In no case has statistical allowance for the factor of regression been made, nor has the principle, which is well known to all competent statisticians, been taken into account in their interpretation of results. Rather they have chosen to pin their faith to a theory of their own which is, briefly, that any environment has a certain determinate degree of stimulative value to the level of which all children who are subjected to it tend to gravitate. Children originally below that level will improve their standing, those above it will lose.

Although this theory has a certain amount of surface plausibility, it should be obvious that before accepting it as valid, the amount of such change that can be charged up directly to the law of regression should first be determined. The statistical procedure by which such calculations can be made is well known; it is surprising that thus far no one has seen fit to apply it. If this were done and it were found that the observed amount of change exceeded that to be expected from regression alone, then, and only then, would further explanation be called for.

Even without such calculations, however, certain points remain to be explained. If the differential gains and losses are due to the leveling effect of the environment, that is, to the tendency of all children to move toward a common level, individual differences within the group as a whole should assuredly be decreased. If, on the other hand, the only thing that is happening is a shifting about of those portions of the individual scores that are due to errors of measurement, the group variability would not be materially changed. Examination of the numerical findings in the studies shows that in none of the instances in which this "leveling" effect is stressed have the standard deviations of the distributions materially changed with the passage of time. For example, page 45 of the monograph describing the deleterious effect upon child intelligence of continued residence in an orphanage judged to be very "nonstimulating" in its effects (6), the statement is made, "The trend for the control group is very clear. Regardless of the original classification, all groups headed for a final classification between 70 and 79 IQ. The effect of long residence for the control children was thus a leveling one, tending to bring all children to high grade feeble-mindedness or borderline classification." But Table 2 of the monograph shows that at none of the three residence periods considered is there a significant change in the standard deviations between initial test and retest, either for the group that was given preschool training or for the control group.

The claims as to the advantages of nursery school training may be checked from another angle. Are these results limited to one nursery school or do others secure similar results? In the 1940 Yearbook of the National Society for the Study of Education, reports from nine well-known child development centers dealt with this topic. Eight of the nine show no effect whatever upon child intelligence as a result of nursery school experience though they do illustrate a number of ways in which a spurious appearance of such results might arise from lack of the essential scientific controls, such, for example, as the use of control groups not attending nursery school but of similar home background and tested at corresponding ages and intervals of time. The one study that purports to show such an effect may be questioned. It may be noted that the mean gain of this nursery school is no greater—the actual figure is slightly lower—than that found for children not in nursery school who were used as controls in other studies previously mentioned. No control group was used in this study. One might also raise a question as to the social value of improving the mental level of the more backward members of the community if this can only be accomplished at the cost of lowering the mentality of the highly gifted! In this study, the mean "loss" in Stanford-Binet IQ for children first testing above 140 was 10.3 points; the mean "gain" for those first testing below 90 was 18.8 points and for the entire group the gain was 4.1 points. If we impute the "gains" of the group with low initial standing to the effect of the nursery school training, logic demands that we ascribe the "losses" of the exceptionally bright children to the same cause. The practical conclusion would then be that bright children should under no circumstances be sent to nursery school for fear of lasting damage to their mentality!

One more point may be raised. This is the possibility of systematic error in testing as a result of the examiners' knowledge of previous test results for individual children and of the group (experimental or control) to which the various children belonged. Since in none of the reported studies is any mention of control of this highly important factor made, it is pertinent to raise the question as to whether any special precautions were taken to guard against errors arising from this source. Anyone familiar with the many special devices employed in all modern psychological laboratories to insure that the results of experiments shall be free from the influence of secondary cues arising either from prejudgment of the stimulus or suggestions as to the expected response, should appreciate the need for utilizing every possible means to make sure that slight variations in testing procedure resulting from unconscious bias on the part of the examiners is ruled out. No one acquainted with the personal integrity of the authors can seriously raise the question of *conscious* intent in the possible modification of results through variation in testing procedure; for my own part, I am confident that no such intent existed. But the administration of an individual test to a small child is not, and cannot be, the formalized kind of procedure that is used with older children, for to make it such would defeat its own object. The testing of young children is an art in which there are many essential variations. In each test given, the aim is to secure from the child the best response to the standardized task that he is capable of giving. In order to allow for differences in child personality, many important details of procedure are wisely left to the judgment of the examiner. For example, the length of time allowed for a given response is usually left unspecified, since too long a delay may be undesirable in the case of a child who is restless and distractible just as too short a time allowance may be upsetting to the one who is slow or timid. The precise amount and character of praise for a child's performance are also left to the judgment of the examiner, for an amount of commendation that is barely enough to keep up the morale of a very shy child or one who is lacking in self-confidence may lead to cocksure carelessness in the case of another. Many other details of test administration are likewise intentionally left flexible enough to provide leeway in which the skilled examiner may adjust his methods to the personality of the individual child. This flexibility of procedure is one of the great advantages of the individual test as compared to the group test. But it also opens the way for many small but systematic variations of which the examiner may be wholly unconscious but that may readily skew experimental results in one direction or another to a degree that only those who have had considerable firsthand experience in the testing of little children and in the training of students in testing can fully appreciate. Obviously, no one can know to what extent this factor has actually affected the results, but certainly the fact that it seems to have been left uncontrolled makes the question of its possible contribution a very pertinent one.

Space does not permit me to make a point-by-point comparison of the evidence and the conclusions that have been drawn from it nor is it necessary that I should do so. This important task has already been performed by McNemar (5), an account of whose findings was published in the *Psychological Bulletin.* The purpose of this brief comment is merely to urge the need for caution on the part of those who might otherwise be inclined to accept the conclusions at their face value and to suggest that before arousing vain hopes in the minds of parents whose children seem less brilliant than their expectations, or attempting to modify legislative precautions in regard to the placement of children for adoption, the facts as well as the theories be given critical consideration.

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Two—The Constancy of Mental Test Performance During the Preschool Period

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Parents are anxious to know at the earliest possible date whether their child is dull, average, or brilliant. How soon can this be determined?

In the case of a child who is to be adopted, it is important that he be able to meet the foster-parents' educational expectations. Can one be sure of the child's ability if he is adopted at one or two years of age?

The following study by Honzik provides some of our best information on this topic. The results of other studies agree quite well with Honzik's findings.

A problem of both practical and academic interest is the predictive value of preschool mental tests. This problem has practical implications for parents and agencies placing children for adoption, and is of academic interest in its contribution to our knowledge of the nature and regularity of mental growth in the early years.

A number of investigations have been made in this field during the past ten years. Results have been reported in the following ways: (a) per cent of the group whose IQ changed by specified amounts on a retest; (b) average change in IQ, and (c) test-retest coefficient of correlation.

Terman's (16) figures show that of children first tested before the age of six years, 14 per cent showed a change of 15 points of IQ or more on a retest; while of children first examined after the age of six, only 4.7 per cent varied to this extent. Johnson (12) reported that in a group of 125 children first tested between two and eight years, 18 per cent showed changes of 10 IQ points or more on retests. The test-retest correlation of IQ for this group was .80. Johnson notes that the children first tested at three years or younger show greater instability in their scores than do the older children. Hildreth (9) found changes of 20 points or more in 19 per cent of the children first tested before the age of six; but only 4.3 per cent of those first tested at a later age changed this much. Goodenough (8) reported that the test-retest correlation for three groups of children (100 cases in each group) aged two, three, and four years was .82 after an interval of 5.9 weeks. Fifty-six of these children were given a third test a year later. The correlations between the first and third tests were .76 for a younger group (age range twenty-four to thirty months); and .84 for an older group (age range thirty-nine to fifty months). These findings suggest that mental test constancy is definitely dependent on the ages of the children tested.

That mental test constancy is also related to the *interval between* tests is to be expected and has been shown in a number of studies. Goodenough (8) reported an average change on a retest of 8.5 IQ points after an average interval of 5.9 weeks; while Mowrer (15) reported an average change in IQ of 13.7 when the interval between tests was a year on the average. This difference, 8.5 points of IQ as compared with 13.7, is probably largely dependent on the difference in the interval between tests, 5.9 weeks as compared with one year. Driscoll (5) reported test-retest correlations after intervals of 6, 12, and 24 months. A slight tendency is noted for the coefficients to decrease with an increasing interval between tests. It seems probable that her results would have been even more clear cut if her findings had been reported for children of uniform age on the first test. The two factors, age at time of testing and interval between tests, were taken into consideration in a study reported by Bayley (3). The 61 children included in her group were tested at one-month and later at three-month intervals during the first three years of life. The children were all tested as nearly as possible on their monthly birthdates. Bayley obtained average test-retest correlations during the preschool period (twenty-one to thirty-six months) of .84 for an interval of three months (21 by 24, 24 by 27, and 27 by 30 months); and a slightly lower average correlation of .83 for an interval of six months (21 by 27, 24 by 30, and 30 by 36).

Consideration of studies that have been made of mental test constancy during the preschool period reveals the need of a study of a fairly large group of children, of known socio-economic status, who would be tested and retested at specified ages during the preschool years. Such a study has been made on a group of children which is representative of the children living in Berkeley, California.

THE SAMPLE

The 252 children included in the present study* were given their first mental test at the Institute of Child Welfare at the age of twenty-one months. At this time the group was divided into two matched subsamples of 126 children on the basis of certain socio-economic factors (parents' nationality, income, father's occupation, socio-economic rating, neighborhood, and mother's age and education). One of these subsamples was admitted to the Institute of Child Welfare's Child Guidance Clinic (under the direction of Dr. J. W. Macfarlane, 14). This group will be referred to as the *Guidance Group;* the children in the second subsample will be referred to as the *Control Group.* The children in the two groups were given mental tests at the following ages:

Guidance	*Control*
21 months	21 months
2 years	———
2½ years	———
3 years	3 years
3½ years	3½ years
4 years	4 years
5 years	5 years
6 years	6 years
7 years	7 years

Every effort was made to test the children as nearly as possible on their birthdays. Actually from 72 to 95 per cent of the children were tested within one month of their birthdates at the various age levels.

As was to be expected in a cumulative study, a number of children were unable to come in for one or more of the mental tests. There appears to be no reason to believe that the children *not* tested at any age level constitute a selected group. The most frequent cause of a missed test was the family's being out of town.

THE MENTAL TEST DATA

Two intelligence tests were used. Each child was tested at successive age levels on the same test, either the California preschool Schedule I (Cal. I) or the California Preschool Schedule II (Cal.

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* This group of 252 children is a representative subsample with respect to certain socio-economic factors of the children included in the Berkeley Survey. The Berkeley Survey is comprised of every third child born in Berkeley over an 18-month period, January, 1928 to June, 1929 (18).

II).* The number of children tested on these scales is shown in Table I. It will be noticed that more tests were given at age level twenty-one months than there are children; this is because 20 children were tested on both scales at that age level.

The mental test scores for each scale reported in this study are sigma scores based on the means and standard deviations of the total point scores at a given age. In some cases, for greater convenience, the sigma scores have been translated into scale scores (Scale score $= 10 \frac{x}{\sigma} + 50$, where $\frac{x}{\sigma}$ is an individual child's sigma score).

The reliability of the two preschool tests (Cal. I and Cal. II) may be shown in the following ways: (a) correlation between scores earned by the same children on the two different tests; (b) split-half reliability coefficients or (c) test retest correlations. Sixty-one children were tested on both Cal. I and Cal. II between the ages of twenty-one months and three years. The correlation between scores earned on the two tests was .71. Split-half reliability coefficients have not been ascertained for this group. However, Bayley (2, 3) reported split-half reliability coefficients for the 60 children in the Infant Growth Study on Cal. I between the ages of twenty-one months and five years. The reliability of the total test (split-half using the Spearman-Brown correction) was:

.83 at 21 months
.80 at 2 years
.89 at 2½ years
.84 at 3 years
.92 at 3½ years
.94 at 4 years
.95 at 5 years

It seems reasonable to suppose that similar split-half reliability coefficients would be obtained for the 252 children in the present study on this scale. Evidence regarding the comparative reliabilities of the two scales will be given when the constancy of test performance on the two tests is discussed.

At six and seven years, all the children were tested on the Stanford-Binet. The IQ's obtained were converted into sigma and scale

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* The published *California Preschool Scale Form A* (11) is composed largely of items from the *California Preschool Schedule I*, together with a few items from the *California Preschool Schedule II*. The test items for the *California Schedules I* and *II* include selections made by Dr. Adele S. Jaffa from several standardized tests [Gesell (6), Minnesota (7), and Bayley (4)], together with some original items first validated at the Institute. These scales have been normed by the Thurstone method of absolute scaling.

TABLE I

Comparison of the Consistency of Mental Test Performance of California Preschool Schedules I and II

									Stanford-Binet	
Age			2-0	2-6	3-0	3-6	4-0	5-0	6-0	7-0
	Test	*n*	*r*	*r*	*r*	*r*	*r*	*r*	*r*	*r*
1-9	Cal. I	181	.74±.03	.65±.04	.58±.04	.59±.04	.41±.05	.39±.06	.32±.05	.30±.05
	Cal. II	73			.46±.06	.35±.07	.34±.07	.42±.07	.17±.08	.26±.08
3-0	Cal. I	153				.77±.02	.64±.04	.60±.04	.57±.04	.57±.04
	Cal. II	94				.64±.05	.46±.06	.54±.06	.56±.05	.54±.05
3-6	Cal. I	132					.82±.02	.73±.03	.63±.04	.59±.04
	Cal. II	94					.69±.04	.68±.04	.71±.04	.58±.05
4-0	Cal. I	121						.71±.03	.62±.04	.59±.04
	Cal. II	102						.71±.04	.61±.04	.58±.05
5-0	Cal. I	108							.67±.04	.69±.04
	Cal. II	104							.76±.03	.76±.03

scores so that they would be in comparable form to the mental test scores obtained between twenty-one months and five years.

The children were tested by experienced examiners, and usually by the same examiner on successive tests.

CONSTANCY OF MENTAL TEST PERFORMANCE

The constancy of mental test performance on the two preschool mental tests, the *California Preschool Schedules I* and *II*, is presented in Table I. Correlations between children's scores at adjacent age levels are fairly high, varying from .64±.05 to .76±.03, but the correlations decrease with the interval between tests. There is only very slight prediction of performance on the Stanford-Binet at six or seven years from an initial test given at twenty-one months (*r*'s from .17 to .32)—and *this is true for both tests* (Cal. I and II). However, the three-year, three-and-one-half-year, four-year, and five-year test scores do give a fair prediction of performance on the Stanford-Binet at six and seven years (*r*'s range from .54±.05 to .76±.03).

The higher intercorrelations of Cal. I scores at the younger ages suggest that this test is a more reliable one between the ages of twenty-one months and four years. However, Cal. II scores at five years are more highly correlated with scores on the Stanford-Binet at six and seven years than are the Cal. I scores. Thus it would seem that Cal. I is a more reliable test and that it has a greater predictive value at the earlier ages, while Cal. II is a more reliable test at the later ages of the preschool period. There is no evidence that the difference in constancy figures for the two tests is due to differences in variability.

Probably the most interesting fact to be gleaned from Table I is the similarity in the trend of the results for the two tests. These two entirely different tests (although similar in form and type of test items) agree in showing that constancy of mental test performance is markedly dependent on both the ages of the children when tested and on the interval between tests.

AGE RATIO

One way in which the dependence of test constancy on interval between tests and age of testing may be shown is to consider the intercorrelations between mental test scores in relation to the age ratio,

$$\frac{\textit{CA at first test}}{\textit{CA at second test}}$$

It is clear that the age ratio decreases as the interval between tests increases. Thus, $\frac{\textit{CA of 1 year at first test}}{\textit{CA of 2 years at second test}} = .50$ is greater than

$$\frac{\textit{CA 1 year at first test}}{\textit{CA 3 years at second test}} = .33.$$

But with the *interval between tests held constant,* an age ratio at a higher age level is greater than an age ratio at a lower age level. Thus, $\frac{\textit{9 years}}{\textit{10 years}} = .90$ is greater than $\frac{\textit{2 years}}{\textit{3 years}} = .66.$

The magnitude of a correlation between tests varies directly with the age ratio, that is, the higher the ratio the greater the correlation. This is shown in Table II. The correlation between 22 age ratios and the corresponding correlation coefficients (for Cal. I) is .92±.02. Thus a high correlation between test scores may be due to a short interval between tests at a lower age level or to a higher age of testing with a longer interval between tests. With the same interval between tests, the correlation will be higher at the higher age levels. It should be emphasized that the relation found here between the age ratio and mental test intercorrelations was for the age range twenty-one months and seven years. It is quite possible that this degree of relationship would not be found to exist for the entire growth period from birth to maturity. However, if the relationship is high throughout the growth period, the age ratio does afford a simple means of recalling the magnitude of the relationship which exists between mental test scores given at different age levels. The correlation between the age ratio and the correlations obtained for Cal. II is .78±.06, which is considerably lower than that found for Cal. I. It is possible that this much lower coefficient invalidates the age ratio concept, but it is also possible that the relationship is lower because the Cal. II scores are less reliable. Inspection of the coefficients obtained for Cal. II shows more inconsistencies than for Cal. I. The *r* having the most effect in lowering the relationship between the age ratio and the *r*'s is the correlation between the three-year and four-year test scores of .46. Higher coefficients were obtained between the three-year scores and scores obtained at five, six, or seven years, suggesting that a higher coefficient was to be expected between the three-year and four-year test scores.

In Table III the constancy of mental test performance for the Guidance and Control Groups is given, regardless of whether children were tested on Cal. I or II. The trends noted when considering results for Cal. I and Cal. II are to be seen again when results for

TABLE II

SCATTER DIAGRAM SHOWING THE RELATIONSHIP BETWEEN THE AGE RATIO AND CAL. I INTERCORRELATIONS OF MENTAL TEST SCORES

	Age Ratio *													*Total*
	.25—.29	.30—.34	.35—.39	.40—.44	.45—.49	.50—.54	.55—.59	.60—.64	.65—.69	.70—.74	.75—.79	.80—.84	.85—.89	f
.80—.84													1	1
.75—.79													1	1
.70—.74										1		1	1	3
.65—.69										2		1		3
.60—.64							1	1	1		1			4
.55—.59				1		3	2							6
.50—.54														–
.45—.49														–
.40—.44				1										1
.35—.39			1											1
.30—.34	2													2
Total f	2		1	2		3	3	1	1	3	1	2	3	22

$* \frac{\text{CA at First Test}}{\text{CA at Second Test}}$

$r = .92 \pm .02$

TABLE III

The Constancy of Mental Test Performance in the Guidance and Control Groups

									Stanford-Binet	
Age			2-0	2-6	3-0	3-6	4-0	5-0	6-0	7-0
	Group	*n*	*r*	*r*	*r*	*r*	*r*	*r*	*r*	*r*
1-9	Guidance	117	.68±.04	.59±.04	.47±.05	.50±.05	.46±.05	.32±.06	.30±.06	.42±.06
	Control	117			.59±.04	.47±.05	.33±.06	.43±.06	.30±.06	.19±.07
2-0	Guidance	113		.71±.03	.69±.03	.60±.04	.46±.05	.32±.06	.47±.05	.46±.05
2-6	Guidance	114			.73±.03	.64±.04	.57±.05	.46±.05	.37±.06	.38±.06
3-0	Guidance	116				.73±.03	.64±.04	.53±.05	.54±.05	.56±.05
	Control	113				.71±.03	.59±.04	.60±.04	.60±.04	.54±.05
3-6	Guidance	107					.78±.03	.72±.03	.59±.04	.63±.04
	Control	108					.74±.03	.71±.03	.62±.04	.59±.04
4-0	Guidance	105						.69±.04	.61±.04	.66±.04
	Control	106						.75±.03	.62±.04	.53±.05
5-0	Guidance	104							.65±.04	.73±.03
	Control	106							.77±.03	.72±.03
S. B. 6-0	Guidance	109								.81±.02
	Control	105								.83±.02
S. B. 7-0	Guidance	104								
	Control	104								

these two different groups of children are compared. The average of the intercorrelations for the Guidance Group is .57 (excluding results obtained at two and two and a half years), and for the Control Group .59. Correlations between scores earned at adjacent age levels are fairly high, varying from .65 to .77. The tendency is again noted for the correlations to vary directly with the ages of the children and inversely with the intervals between tests. Thus the correlations for the Guidance Group over the following two-year intervals are:

2 to 4 years = *.46*
3 to 5 years = *.53*
4 to 6 years = *.61*
5 to 7 years = *.73*

There are several slight inconsistencies in the results obtained for the two groups. A higher correlation was found between the twenty-one-month and three-year tests for the Control Group (.59), than for the Guidance Group (.47). The reverse might have been expected since a larger proportion of the Control children were tested on the less reliable Cal. II. However, it seems that the coefficient for the Guidance Group is too low partly because of chance factors, since a higher correlation was obtained between the twenty-one-month and three-and-one-half-year test than between the twenty-one-month and three-year test. Another inconsistency occurs in the correlation between the twenty-one-month scores and those obtained at seven-years (.42). The comparatively large standard deviation of the seven-year test scores may account in part for this coefficient.

The correlation of .30 between the twenty-one-month and six-year mental test scores indicates the slight prediction that exists over that age period. Updegraff's (17) and Goodenough's (8) finding that an initial test is less reliable than a subsequent test should be recalled when considering these results. However, the same trend is noticeable when performance at two and two and a half years (which are not "first tests") are compared with those at four, five, six, and seven years.

The correlation between the age ratio and intercorrelations of mental test scores for Guidance children (36 *r*'s shown in Table II) is .87±.03. The correlation between the age ratio and correlations obtained for the Control Group is .91±.02. These correlations between the age ratio and constancy coefficient for the two tests (Cal. I and II) and for the two groups (Guidance and Control) agree fairly well in showing that the age ratio may prove to be a useful concept in relation to group mental test constancy figures.

An interesting consideration is whether the correlations between mental test scores are determined by a relatively small proportion of cases or by the group as a whole. Distributions of the changes in mental test scores between certain age levels are given in Table IV.

TABLE IV

RELATION OF CHANGES IN MENTAL TEST SCORES TO THE INTERVAL BETWEEN TESTS

	Interval Between Tests									
Change in Sigma Score	*1 Year 7-6 yr.*		*2 Years 7-5 yr.*		*3 Years 7-4 yr.*		*4 Years 7-3 yr.*		*5 Yrs., 3 Mos. 7 yr.-21 mo.*	
	n	*%*	*n*	*%*	*n*	*%*	*n*	*%*	*n*	*%*
3.5			1	*1*					1	*1*
3.									2	*1*
2.5							1	*0*	5	*3*
2.					1	*1*	6	*3*	8	*4*
1.5	3	*1*	6	*3*	17	*9*	18	*9*	12	*6*
1.	16	*8*	28	*14*	23	*12*	24	*12*	16	*8*
.5	48	*24*	39	*20*	38	*20*	35	*17*	33	*17*
0.0 (—.2 to .2)	62	*31*	64	*32*	41	*21*	41	*20*	40	*20*
— .5	52	*26*	34	*17*	32	*17*	38	*19*	27	*14*
—1.0	18	*9*	22	*11*	25	*13*	29	*14*	27	*14*
—1.5	2	*1*	4	*2*	11	*6*	8	*4*	10	*5*
—2.0					5	*3*	1	*0*	10	*5*
—2.5	1	*0*							3	*2*
—3.0									2	*1*
Total	202	*100*	198	*100*	193	*102*	201	*98*	196	*101*
Av. change in sigma score	.47		.56		.71		.72		.92	

It may be seen in this table that the tendency to change in score is a general one. The per cent of cases showing a very slight change on a retest varies from 20 per cent between twenty-one-months and seven years to 31 per cent between six and seven years. The average change in score between six and seven years is .5 of a sigma,* while the average change in score between twenty-one months and seven years is .9 of a sigma score. There are a few instances of marked changes in score but the distributions of changes in scores are normal and suggest a multiplicity of factors determining the changes in mental test performances.

Examples of varying degrees of constancy of mental test performance for individual children are shown in Figure 1. Case 355 and Case 385 (who earned the highest IQ in the group at six years) represent instances of extremely marked gains in relation to the group scores between twenty-one months and six years. Case 355 increased from the 1st to the 90th percentile (assuming that the distribution of sigma scores is normal at the various age levels);

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* The standard deviation of the six-year and seven-year *IQ's* is 13 points of *IQ*. Thus an average change in sigma score of .5 may be considered to equal 6.5 points of *IQ* approximately.

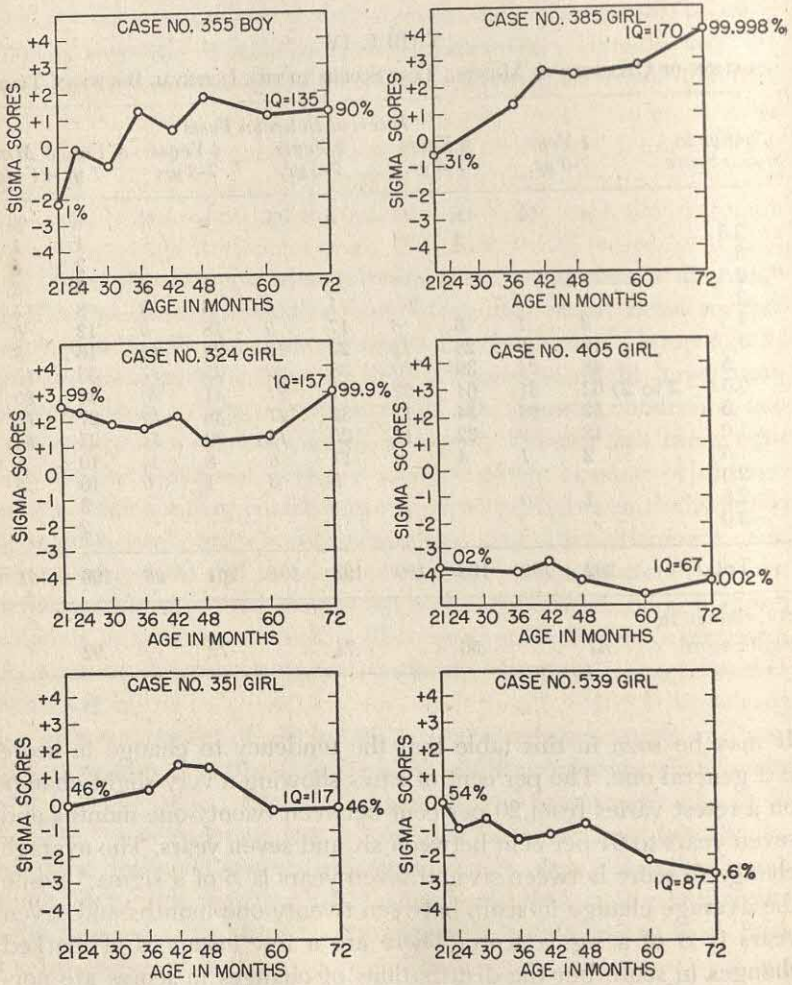

Figure 1. Constancy of mental test performance for individual children.

Case 385 increased from the 31st to the 99.998th percentile. Case 324 in contrast consistently earned one of the highest scores in the group. It is interesting to note how comparatively little her scores vary on the eight different tests. Case 405 is a microcephalic child born to a superior family. She was diagnosed a microcephalic at the age of twenty-one months. Her scores have been the lowest in the group at every age level.* Case 351 represents a fluctuating performance. Other cases could be presented to show a drop in test score followed by a gain. Case 539 score above the average at twenty-one months but obtained the second lowest score in the group at six years. Her steady decline in mental test performance with reference to the group is disturbingly impressive.

The constancy of mental test performance for children earning extremely high and extremely low scores at twenty-one months and at six years have been considered in some detail. Of the 10 children who obtained the lowest scores at twenty-one months, only two were among the 10 who obtained the lowest scores at six years (the microcephalic Case 405 was, of course, one of these and she was the only case of the 10 who failed to show some gain between twenty-one months and six years with reference to the group scores). The average sigma score of these 10 children increased from –2.4 sigma at twenty-one months to –.9 sigma at six years. The greatest improvement for this group of 10 children occurred between twenty-one and twenty-four months. Factors which may be contributing to the marked changes in scores between the first two tests are (a) unreliability of the initial test, due to emotional or other factors, (b) changes in the nature of the test items, that is, a larger proportion of test items requiring the comprehension and use of language at twenty-four than at twenty-one months. The greatest gain in this group of children who earned such low scores at twenty-one months, was shown by Case 355 (Figure 1) with an increase of 3.5 sigma. The average IQ of the 10 children earning the lowest scores at six years was 92. The scores of these children decreased rather gradually from an average sigma score of –1 at twenty-one months to an average sigma score of –2.1 at six years. The scores of two of the 10 children decreased over 2 sigma.

Two of the 10 children earning the highest scores at twenty-one months were among the 10 highest at six years. Thus, for both the low and the high group, two out of 10 (or 20 per cent of the children) maintained their extreme positions during the period under consideration. Case 324 (Figure 1) is one of those who maintained her extremely high scores. The average scores of these children dropped from 2.2 sigma to 1. sigma, and as in the case of the children with the lowest scores, the greatest change took place between twenty-one and twenty-four months. Two of the 10 children showed a decrease of 2.8 in sigma score.

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* Case 405's scores have not been included in any of the correlations reported in this study. It is not to be expected that such a markedly deviating case would be found in a sample of 252 cases. If her scores had been included in the correlations it would have been necessary to report the coefficient "including" and "not including" her scores. The inclusion of her scores would, of course, have raised the correlations reported between mental test scores earned at different age levels.

The 10 children with the highest scores at six years increased from an average of .6 sigma to 2.4 sigma between twenty-one months and six years. This increase took place quite gradually from one age level to the next. Case 385 (Figure 1) showed the greatest gain (4.6 sigma).

Discussion of the constancy of mental test performance for children earning extreme scores shows that marked changes occur over this age period, and indicates further that the changes are most apt to occur between twenty-one and twenty-four months. In other words, the initial test does seem to lack the predictive value of a second test given fairly soon after the first.

The results reported on the constancy of mental test performance for children earning extreme scores show that there is a strong tendency for a regression toward the mean on retests. The correlation between high scores on the first test at twenty-one months and gains in mental test scores between twenty-one months and six years is −.68. Thus, for a child earning an extreme score at twenty-one months, there is more likelihood of a regression toward the mean than constancy of performance in the subsequent years.

SUMMARY

A group of 252 children who comprise a representative sample of the children living in Berkeley, California, were given mental tests at specified ages during their preschool years. The children were first tested at the age of twenty-one months, and subsequently on their half-yearly and yearly birthdates. The constancy of mental test performance between twenty-one months and seven years was found to be essentially the same on two different preschool mental tests (*California Preschool Schedules I* and *II*), and for two comparable subsamples of the main group. Although test performance is fairly constant over short age periods, mental test constancy appears to be markedly dependent on both the ages of the children and the interval between tests. The age ratio,

$$\frac{\textit{CA at first test}}{\textit{CA at second test}}$$

which takes both these factors into account, is correlated with the mental test constancy coefficients on the *California Preschool Schedule I* to the extent of .92±.02; and with test constancy coefficients on the *California Preschool Schedule II* .78±.06. The results show that an initial test given at the age of twenty-one months gives a negligible prediction of test success on the Stanford-Binet at six or seven years. Tests given at three years or later to children who have had test experience are much more predictive of success on the Stanford at six or seven years (correlations range from .58 to .76), yet are not high enough to warrant full confidence if one were adopting a child. Marked individual differences in test constancy are noted. Twenty per cent of the children earning extremely high or extremely low scores at twenty-one months maintain that position to the six-year test. On the other hand, there are many instances of extremely marked changes in mental test scores. One child gained as much as 4 sigma, while two children gained over 3 sigma between twenty-one months and six years.

These results suggest the impossibility of making an accurate prognosis of future ability on the bases of a single mental test given before the age of two, but suggest further that repeated tests and tests at later ages of the preschool span have increasing predictive value.

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Three—The Intelligence of Isolated Mountain Children

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Every intelligence test that has been devised requires the subject to respond to test materials. The designers of tests attempt to choose materials that are almost universally present. At early ages this is possible. In every group the young child learns the names of the parts of his body, the distinction between morning and evening, simple counting, and the distinction between right and left. As the child grows older, however, what he learns varies greatly from society to society, and it becomes increasingly difficult to find test questions that do not penalize some groups.

Sherman and Key showed that even English-speaking children may be penalized in intelligence tests by the poverty of their environment.

This report of the results of intelligence tests made of mountain children is part of a larger study begun in the summer of 1929 to determine the cultural influences which affect intellectual, emotional, and personality development and the influences determining the attitudes of mountain people living in relative degrees of isolation.

The communities studied were four hollows located approximately 100 miles west of Washington, D.C., in the Blue Ridge Mountains and a small village at the base of the Blue Ridge about the same distance from Washington to the southwest. The Hollows were settled in the precolonial period by English and Scotch-Irish immigrants. When German immigrants were given most of the land in the Shenandoah valley surrounding these mountain ranges the English and Scotch-Irish people were forced up the mountainside. The topography of this region is such that the settlers were forced further within the mountains, settling in hollows surrounded by mountain ranges. There they built their log and mud cabins, many of which still remain and are inhabited. Each of the hollows selected for study, Colvin, Needles, Oakton, and Rigby are close to each other but are separated by comparatively high mountain ranges. Of these hollows, Colvin is at the lowest level in social development. This hollow is small, consisting of a small number of families living in scattered, mud-plastered log huts. There is no road, except for a trail to the outside world. One small log and mud cabin is rented by the county school board for a school. There is no general meeting place and the church meetings which have been held in the past have been discontinued except for a very occasional revival meeting. With three exceptions, the adults are illiterate. They are descendents of the original settlers who married relatives and mixed very little with the people outside of the hollows. Colvin Hollow is so named because most of the inhabitants are Colvins. Many of the younger children do not know their last names. They identify themselves, for example, as Sadie's Bennie or Dicy's Willie.

Needles Hollow adjacent to Colvin Hollow, is next in the scale of social development. It is reached by a rocky road from a small hamlet at the base of the mountains. Its patches of ground, from two to five acres on the average, surrounding the cabins, approach the status of small farms. It is a more socialized community and many of the adults are literate. The children have had good school advantages compared to Colvin Hollow.

Oakton Hollow, next higher in the social scale, is separated from Colvin Hollow by a high mountain. The road to the valley is passable for old Fords and wagons most of the year. The hollow boasts of a combined general store and post office and many of the inhabitants receive mail and an occasional magazine. There exists a greater social consciousness than in Colvin or Needles Hollows. Oakton Hollow has had about four months of school each year for some time. The people are fairly prosperous although they have but little surplus farm products to sell in the valley.

Rigby Hollow, culturally further developed, can be reached from the valley much more easily than any of the other three hollows. The present school was established by missionaries about nine years ago and has been conducting regular school terms. Church and Sunday School services are held regularly. The farms are larger than those of the other hollows and there nearly always is a surplus which is sold in the valley. School terms have been about seven months each year for the past eight years and approximately 75 per cent of the inhabitants are literate.

For purposes of comparison a small farm and sawmill town, Briarsville, was chosen. It is located at the base of the mountains to the south of the Hollows. The town has a hard-surfaced road connecting it with the principal cities of Virginia. The school building is a modern structure with four classrooms, three of which are used regularly. The school board employs three well-trained teachers. The town has a good general store, telephones, and receives newspapers.

The comparison of the intelligence test results of the mountain children with those of the children of Briarsville is especially significant in view of the origin of many of the residents of this town. Many of the inhabitants migrated from the mountains in the past to obtain work on the adjacent farms and in the sawmill. At first socially isolated from the "first" families of this town, the children now mingle freely. It was thought that a comparison of intelligence test results of the mountain children with those of Briarsville would be much more significant than with children of an average town or city.

Intelligence tests were given to more than half of the children of the four mountain hollows and Briarsville. Not every child was tested, for some of the younger children could not be taken to the place where the tests were given, and a few of the others refused to cooperate. Nine tests were used: the Stanford-Binet; The National Intelligence Test, Scale B, Form 2; Pintner-Cunningham Primary Mental Test. For performance tests the following were employed: Manikin, Seguin Form Board, Mare and Foal, Healy Puzzle "A," the Knox Cube Test from the Pintner-Patterson scale of performance tests, and Goodenough's Drawing of a Man.

A representative sample of the school population thus was tested. A total of 386 tests were given to the children in the mountain communities and 198 in Briarsville. The children in Briarsville were not given Stanford-Binet tests because of the difficulty in organizing the program there. One hundred and two children were examined in the mountain communities and 81 in Briarsville. In addition to an investigation of test scores and mental age results, a qualitative analysis of the responses was made.

Table I shows the length of the school term in five communities since 1918. The Colvin Hollow School has been the most irregular with the least number of months of schooling. Between 1918 and 1929 Colvin Hollow has had only 16 months of school at irregular intervals. It was impossible to find an exact record of the years in which school was held. Rigby Hollow has the most regular school term of the mountain communities.

The superiority of the Briarsville school system is shown not only in the greater number of months in which school was held in the past 11 years but also in the regularity of the school term.

TABLE I

LENGTH OF SCHOOL TERMS IN MONTHS IN THE FIVE COMMUNITIES

Year	*Colvin*	*Needles*	*Rigby*	*Oakton*	*Briarsville*
1918-19	16 months	?	4	6	9
1919-20		2	4	6	9
1920-21		2	4	6	9
1921-22		0	4	6	9
1922-23		4	4	7	9
1923-24		2	4	5.5	9
1924-25		2	7	6	9
1925-26		2	7	4	9
1926-27		2	7	5	9
1927-28		2	7	3	9
1928-29		5	7	7	9
1929-30		7	7	7	9
Total months 1918-1930	16	30?	66	66.5	108

Table II shows the average intelligence quotient of the children in the four mountain communities and Briarsville. The average intelligence quotient of the Briarsville children was higher than that of the mountain children in every test, and had a smaller standard deviation. The results give further evidence of the effect of systematic training upon intelligence test ratings, a factor often slighted in comparative studies of intelligence test scores.

TABLE II

AVERAGE INTELLIGENCE QUOTIENTS ACCORDING TO VARIOUS TESTS

Tests	*Mountain Communities*			*Briarsville*		
	Number of cases	*Average*	*S. D.*	*Number of cases*	*Average*	*S. D.*
Stanford-Binet	32	61.5	11.2			
National	24	61.2	17.5	50	96.1	15.2
Pintner-Cunningham	42	75.9	17.1	31	87.6	13.0
Four performance tests:						
Year scale	54	83.9	24.8	10	118.6	17.1
Med. M.A. scale	54	79.1	23.8	10	95.6	16.3
Drawing of a Man	63	72.3	17.9	67	76.3	17

The dependence of the intelligence quotients on the kind of test used is shown in a comparison of the average IQ's of the mountain children on the different tests used. The highest average intelligence quotients are found in the test presumably most independent of language and of school training, and lowest in those utilizing language ability.

When we examine the results of the tests in Briarsville, on the other hand, we find that while the highest average IQ was obtained in the performance tests, the next highest was on the National Intelligence test—a test dependent upon language ability. This may be additional evidence that systematic and consistant training in a community of a comparatively high order or social organization is a stimulus to the development of the kind of intelligence we ordinarily measure by tests.

These mountain children are slow and cautious with a slow tempo of response. The way in which the environment influences a child's method of responding probably has not been studied sufficiently in intelligence test results. In scoring the results it was found that the children rated highest in those tests in which the tempo of the directions and the responses was slowest. It is not surprising to find that the children rated highest on tests which took into account least the factor of speed. The children in these mountains live in an environment which does not put a premium on speed and the problem of evaluation of their test scores thus is complicated further.

The Stanford-Binet test at once might be considered inadequate because of its evident dependence upon language and school training. Analysis of the successes and failures on this test further showed its unadaptability for studying this type of children. Failures were most evident on items involving abstract comprehension. This sort of failure differed in degree in the various communities. The Colvin Hollow children failed most frequently in tests involving calculation, in part because the terms used were foreign to them. The difficulty of evaluating failures on simple problems is due in part to the uncertainty of knowing whether the children failed because of insufficient language comprehension to understand the directions. Rote memory was found to be above the average of other test results, but the most common failure in Colvin Hollow was in the reversal of numbers. Following the giving and scoring of the tests, a number of children were given practice in the reversal of the number sequence 1-2-3. After it was thought that they could reverse this sequence other numbers containing four figures were given. As an example, the sequence of 6-5-2-8 was reversed by most of the children as 6-5-4-3-2-1.

The almost universal failure of the mountain children in the ball and field test indicates their lack of ability to comprehend and solve a simple problem involving foresight and planning ability. Few of the children appeared to have a plan for finding the ball in a circular field. Usually a line was drawn in the center of the diagram and in some cases this was varied by dots indicating trees. Many children of Colvin and Needles Hollows could not understand the directions of the ball and field test. They had little comprehension of the meaning of "field" and were astonished at a ball being lost. (Most of the children never had seen a ball.) One boy of thirteen made a curious effort. He drew a number of small rough circles in the enclosure which he explained as representing trees. Then he drew a line from one circle to the next connecting them. He then stated that he was hunting for the lost article under the trees. In this and other cases it was very difficult to assume, as one is forced to do in scoring the test, that the failure indicates a deficiency of innate intelligence even on that one test. Although it is not assumed that a child must have experience in the performance called for on a given test, and indeed such direct experience would mitigate the significance of the score, it can be assumed that a child must have had some approximate or similar experience. These mountain children live in an environment calling for little planning and ingenuity expected of an average child of not more than nine years.

Evidently space and form differentiation as employed in these tests are relatively foreign to these children. Only one of the younger children in Colvin Hollow correctly copied the drawing of a diamond.

The items which the children in all the hollows passed most consistently were the mutilated pictures, counting backwards from 20, arranging weights and comprehension of pictures. In two of the hollows most of the children could not name the days of the week in correct sequence. This failure probably was due to the fact that they have no use for differentiation of days, since one day is like the next in its significance except for the days of going to school and staying at home.

Table III gives the average intelligence quotients on various tests according to increasing chronological age. It shows a decrease in intelligence quotients with increase in chronological age for every test except the National, applied to the mountain children. The decrease in the intelligence quotients in some of the tests is as great from the sixth to the tenth year as from the tenth to the sixteenth

TABLE III

AVERAGE INTELLIGENCE QUOTIENT ON FIVE TESTS ACCORDING TO INCREASING CHRONOLOGICAL AGE*

Chronological Age	*Number of Cases*		*Pintner-Cunningham*		*National Intelligence*		*Drawing of a Man*		*Performance Scale*			
									YEAR SCALE		MED M. A. SCALE	
	Mountains	Briarsville	Mountains	Briarsville	Mountains	Briarsville	Mountains	Briarsville	Mountains	Briarsville	Mountains	Briarsville
6-8 …………	12-13	8	84	94			80	93	91		89	
8-10 …………	15-23	4-22	70	91		117	66	82	84	119	76	93
10-12 …………	5-16	5-20	53	76	66	101	71	69	86	108	70	87
12-14 …………	7-12	16			67	91	69	73	83		83	
14-16 …………	8-15	14			52	87	49	70	75		73	

* The figures indicating the number of cases does not mean that every test was given to the numbers indicated. The minimum and maximum number of children given a test at the respective chronological ages is shown.

year. In some cases the decline in intelligence for children over ten is greater than for children between six and ten. An intelligence test is an indirect measure. An estimate of intelligence is based on the information the child has been able to obtain. In the mountain environment increments of information become less large with increases in age, and the seven-year-old has relatively more chance to gather information and to learn by experience than the twelve-year-old in the same environment.

SUMMARY

The results of the intelligence tests of mountain children living in varying degrees of isolation appear to corroborate the belief of many psychologists that the expression of intelligence, as measured by standardized tests, depends in a large measure upon the opportunities to gather information and upon the requirements made upon the individual by his environment. Since the ancestry of the children of all the Hollows came from the same stock, the claim cannot be made that some of these mountain people are "degenerate" and therefore their children are expected to be retarded intellectually, a claim too often advanced for the supposed inferiority of isolated mountain children. Futhermore, as has been shown in this paper, the young children of the various Hollows do not differ greatly in intelligence, whereas great differences are found between the older children of the different Hollows. The only plausible explanation of the increasing difference with increasing age is that children develop only as the environment demands development. The Colvin Hollow environment is as stimulating to the child of four or five as that of Oakton or Rigby, but Colvin Hollow requires relatively little more of its older children whereas Rigby Hollow requires an ability for social adjustment met only by a high order of intelligence.

Four—A Survey of Studies of Identical Twins Reared Apart

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Identical twins have identical genes. Therefore, any differences between two identical twins are due to environmental influences such as birth injury and disease, or to differences in education and social environment. It is of particular interest, therefore, to study identical twins who have been reared in different environments. They show us, in effect, how the same individual *develops under two sets of circumstances. No other situation can provide such information.*

Nothing is more certain, after a little consideration, than the statement that heredity and environment are coacting factors in the development of any living individual and that both are absolutely essential. If the individual's heredity potencies could somehow be annulled he would immediately lose all physiological and mental characteristics and would remain simply a mass of dead matter. If he were somehow deprived of all environment, his heredity potencies would have no scope for their activity and, once more, he would cease to live. To ask whether heredity or environment is more important to life is like asking whether fuel or oxygen is more necessary for making a fire. But when we ask whether the *differences* between human individuals or groups are due to their differing heredity or to differences in their present and previous environments, we have a genuine question and one of great social importance. In a broad sense both heredity and environment must be concerned in causing individuals to be different in ability and personality, but it is a real question whether to attach more importance to the one or the other and whether to look to eugenics or euthenics for aid in maintaining and improving the quality of the population.

TWINS

Though the investigation of human heredity is beset with difficulties, due in part to the long interval between generations and in part to the lack of well-controlled matings such as are used by the geneticist in his experiments on plants and animals, enough is known to justify the carrying over of certain fundamental laws and concepts. We know that certain human traits are dominant, and certain others recessive. We know that certain traits are genetically simple, and many others complex. We know, too, that in any mixed population—and every human population is decidedly mixed in comparison with the "pure lines" of the geneticist—it is practically impossible for two matings of the same parents, even, to result in offspring having the same heredity. The chromosomes of the parents are combined differently in their several children. The same parents can produce a considerable variety of children, though the variety is not so great as it is in the population as a whole.

But it sometimes happens that a pair of twins comes from a single fertilized ovum, that is, from a single ovum fertilized by a single spermatazoon. The embryo in this case, after starting life as a single individual, divides at a very early stage of development and gives rise to two individuals. Now as all daughter cells derived from the same fertilized ovum have exactly the same assortment of genes, the two (or sometimes more) individuals derived from the same fertilized ovum are exactly alike in genic constitution. They are truly identical as far as heredity is concerned. They are often indistinguishable in appearance. They are necessarily of the same sex. These "identical" or monozygotic twins thus afford perfect material for the study of the differentiating effects of environment. Since they are genetically identical any difference which develops between them must be due to some sort of environmental factor.

The remaining twins, called fraternal or dizygotic, are derived from two different eggs or ova, fertilized by two different spermatozoa. Genetically they are no more alike than other siblings. Being conceived and born at the same time does not make them any more alike in heredity. But their environment, both prenatal and postnatal, must on the whole be more alike than that of other siblings.

IDENTICAL TWINS REARED APART

The first pair of separated identicals to be tested and carefully studied was that reported by H. J. Muller (2). The principal series of such pairs was gathered with much labor over a period of years by the Chicago group, Newman, Freeman and Holzinger, and reported in their book on *Twins* in 1937 (3). They discovered, measured, and tested 19 pairs of identicals who had been separated in infancy or early childhood and reared in different families and communities. Another pair has been added by Gardner and Newman (1).

TABLE I

Some Data from Identical Twins Reared Apart. (Newman, Freeman and Holzinger; Muller; Gardner and Newman; Saudek

				Environmental Differences			
Case Number	*Sex*	*Age at Separation*	*Age at Testing*	in Years of Schooling	in Estimated Educational Advantages	in Estimated Social Advantages	*IQ Difference*
11	f	18 mo.	35	14	37	25	24
2	f	18 mo.	27	10	32	14	12
18	m	1 yr.	27	4	28	31	19
4	f	5 mo.	29	4	22	15	17
12	f	18 mo.	29	5	19	13	7
1	f	18 mo.	19	1	15	27	12
17	m	2 yr.	14	0	15	15	10
8	f	3 mo.	15	1	14	32	15
3	m	2 mo.	23	1	12	15	− 2
14	f	6 mo.	39	0	12	15	− 1
5	f	14 mo.	38	1	11	26	4
13	m	1 mo.	19	0	11	13	1
10	f	1 yr.	12	1	10	15	5
15	m	1 yr.	26	2	9	7	1
7	m	1 mo.	13	0	9	27	− 1
19	f	6 yr.	41	0	9	14	− 9
16	f	2 yr.	11	0	8	12	2
6	f	3 yr.	59	0	7	10	8
9	m	1 mo.	19	0	7	14	6
Muller	f	1 mo.	30	9	?	?	− 1
Gardner and Newman .	f	1 mo.	19	0	2	?	− 3
Saudek	m	1 mo.	20	0	?	?	± 4

The estimated differences in educational and social advantages are in "points" with a maximum possible of 50. From the case material, each of five judges rated the environmental differences between every pair of twins on a scale of 10 points, and the figure given in the table is the sum of these five ratings. A minus sign before an IQ difference means that the twin who received the higher rating for educational advantages obtained the lower IQ.

And a British pair, reared apart up to the age of nearly ten years, has been studied by Saudek (4). Diagnosis of monozygosity was carefully made in all these cases.

Table I presents certain data for each of the separated identicals. With regard to prenatal and natal environment, the case histories of the pairs numbered 1 and 8 suggest the possibility that the twin with lower IQ in each of these pairs got a poor start and perhaps suffered a slight but permanent handicap.

For the most part the environment in which the twins of the same pair were brought up did not differ extremely—not more than would be true of children brought up in the same community. In a few instances the difference was rather large. The greatest difference in education occurred in the case of two girls one of whom was reared in a good farming region and who went through college and became a school teacher, while her twin grew up largely in the backwoods and had only two years of regular schooling. This girl however obtained employment later in a large city and became a general assistant in a small printing office where she performed a variety of duties including typesetting and proofreading. On being tested her IQ came out at 92, while that of her college-educated twin sister was 116. This difference of 24 points in IQ was the largest found in any case.

Another pair of young women had been separated in infancy and one had been reared on a good farm and had gone to school only for the eight grades, while the other had lived in a small town, gone through high school, studied music and engaged in clerical work. The town girl's IQ as determined by the test was 106 while the farm girl made only 89. In spite of this large difference in the tests the country girl gave the impression of being fully as intelligent, or competent, as her twin sister.

A pair of young men from Tennessee had been brought up, the one in a small town where he went through high school and engaged in business, the other back in the mountains with irregular schooling amounting to eight grades at the most. Tested at the age of twenty-five, the mountain boy obtained an IQ of 77, the town boy of 96.

TREATMENT OF DATA

There are two main ways of treating the data from separated identicals. The more obvious way is to compare them with identical twins reared together, as can be done either by comparing the mean difference between twins in the one class with the corresponding difference in the other class, or by comparing the correlation between the paired individuals in the two classes. Either method is essentially a study of intrapair variance. The difference has to be taken without plus or minus sign, because in the identicals reared together there is no known differentiating factor to give either twin a distinctive place in the pair. Because the intrapair difference is taken without sign, it needs to be corrected for the chance error of observation.

The other way of treating the data is to look for environmental factors that might differentiate the members of a separated pair and to determine whether a given factor has produced a significant difference between the favored and the disfavored twins taken as a group. For example, if it can be seen that one twin has received better educational advantages than the other, in each pair we can determine whether there is a significant difference in IQ in favor of the better educated twins. Here each difference has a sign, plus or minus, and the average difference (account being taken of the signs) does not need to be corrected for chance errors of observation. It is important to notice this distinction between the two methods of treatment. Suppose the mean difference between identicals to be 5 points IQ; this difference does not exceed the error of observation and therefore does not indicate any true difference. But suppose, in a sample of identicals reared apart, the better educated twin averages 5 points higher in IQ than the other. This may be a significant difference (given an adequate sample), for the chance errors of observation would not favor either class of twins as against the other class.

Taken without regard to sign, the average IQ difference between separated identicals is 7.6 points. Correction for chance errors of observation would bring this difference down to 6 points net, a figure to be compared with the estimated net difference of 3 points between identicals reared together, and of 15 points or more between children paired at random from the same community. It is probable, then, that environment did make these separated twins differ in tested intelligence, though not to any such extent as obtains among the children of a community.

When the IQ differences are averaged with account taken of sign, the twin having the advantage in educational opportunities usually surpasses the other. On the average the IQ was 6.0 points higher for the better educated twin. This difference is statistically reliable, being over three times its standard error. It seems safe to conclude that when one of a pair of identicals has been afforded better educational advantages than the other, the better educated one will on the whole do better in the intelligence tests.

However, there were only six pairs differing very much in the amount of formal schooling received. These six show an average difference of 13 points in IQ. The remaining pairs show only a small and unreliable IQ superiority for the better educated twin (3 points on the average, and only 1 point when the two cases of possible prenatal handicap are omitted). It appears, then, that rather a large educational advantage is required to give any dependable superiority in tested intelligence.

Years of schooling are of course not an adequate measure of educational advantages. Newman (3) and his colleagues endeavored to do a little better by going over the case histories of their 19 pairs and estimating the educational difference between the twins by aid of a rating scale. The ratings are given in our table. There is a correlation of + .79 between the estimated educational difference and the obtained difference in IQ. This substantial correlation depends largely but not wholly upon the few pairs whose education was very unequal.

CONCLUSIONS FROM THE TWIN STUDIES

If we consider the results on the intelligence of identical twins reared apart, two conclusions seem probable even though the sample is still far too small to make either conclusions sure. In the first place, radical differences in education can create substantial differences in intelligence, so far as intelligence is measured by our tests. Differences in IQ as great as the standard deviation of the population have been found in several instances, corresponding to large differences in educational advantages. We can conclude that the educational environment, taken in a broad sense, has a marked effect on such intelligence as we are now able to measure.

In the second place, however, the differences between identical twins reared apart are remarkably small except in those cases where the contrast of educational advantages was very great. For the majority of the separated identicals the IQ difference was no greater than for identicals reared together. When individuals of identical heredity are subjected to environments differing about as much as those of the children in an ordinary community, such identical twins differ much less than the children of such a community. Therefore the differences found among the children of an ordinary community are not accounted for, except in small measure, by differences in homes and schooling. To repeat—if the differences in intelligence found among the children of a community were mostly due to differences in home and school environment, these differences would remain almost in full force even if the heredity of all the children were made identical. But when a trial is made of this hypothesis by placing identical twins in different families not too different in environment, the twins show only a small fraction of the difference found in the community at large.

These two statements—(a) that differences in environment can produce substantial differences in intelligence, and (b) that the differences actually present in a community are *not* due mostly to differences in environment—may appear mutually contradictory. That they are not contradictory has been emphatically pointed out by several students of the nature-nurture problem.

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Five—Is Mental Resemblance Related to Physical Resemblance in Sibling Pairs?

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In brief, as the title indicates, this study is concerned with the following question: If siblings are alike in appearance, does this indicate a mental similarity as well? Or, put another way, if a child resembles a parent or other relative physically, is he likely also to resemble him in intellectual traits?

Of course, identical twins are excluded from such a study since it is known that in the case of such twins, the genetic similarity is thoroughing, that is, it extends to genes controlling psychological as well as physical traits.

The conception that mental processes are closely related to physical structures, particularly of the central nervous system, is necessary unless we wish to imagine ourselves as completely dualistic beings in which mind and body pursue independent paths that meet only by coincidence. Although most experimental attempts to establish the presence of correlations between normal variations in mental and structural traits have failed, the results far from disprove that close interdependence exists. They only demonstrate that the problem is a resistant one, and that associations have not yet been sought in the domains where they actually reside.

Phrenology and various other "systems" of character analysis are the outlawed progeny of dignified speculations that began in the armchairs of philosophers, and continued in the modern laboratories of psychologists. If logical grounds alone do not answer the bizarre claims of these "systems," such experiments as those of Paterson and Ludgate (10), Kenagy (7), Cleeton and Knight (1), Sheldon

(12, 13), and Hull and Evans (5) do so most adequately. These investigators have reported zero correlations between reliable pooled ratings of personality traits and the features of head and face on which various "systems" pin their faith.

Pronounced relationships between anatomical structures and mental and personality characteristics will probably never be found as long as they are sought with utter disregard of the intrinsic functions of physical organs. To expect generosity with a wide mouth, or a calculating disposition with small ears, is as reasonable as to look for the cause of color-blindness in the alimentary canal. While the work of Kretschmer upon morphological types might seem to offer an instance of correlation between traits having no intrinsic connection, the explanation of his results probably lies in the endocrine secretions that affect temperamental as well as morphological patterns.

It is not unlikely, however, that genetic linkages may be discovered within family groups between disparate human traits whose only bond in common is the chromosome in which their determiners lie. Many such linkages have been found between unit characters in the lower animal forms, especially in *Drosophila.* With the exception of "sex-linked" characters such as hemophilia or color-blindness, however, no clear case of simple linkage has yet been reported in man. Since there are only a few human traits which behave as unit characters, one's best chance of discovering linkages is probably to examine variable traits determined by several or many genes.

PROBLEM

The present study investigates the possibility that some of the genes for mental ability and general physical appearance are linked. There is a widespread impression that this is the case. In lay circles in which the principles of genetic inheritance are unknown, the comment is frequently made that this child "takes after" his father's side of the house, that another child is "exactly like his mother's people," while still another "is just his older brother all over again." The family commonly expects these children to grow up with the mental and personality traits of the relatives they most resemble in appearance, and is fearful if a child in its midst looks too much like some unfortunate black sheep whose portrait has been turned to the wall.

There is a sound theoretical reason why the amount of physical resemblance which a child bears to his father or to his mother should not influence in the least the amount of his resemblance to either parent in hereditary mental traits. Each parent always contributes half of the 48 chromosomes which determine within more or less narrow limits the course of a child's mental and physical development. If the child happens to inherit, for example, the brown eyes of one of his parents, this does not mean that the child has received a "larger share" of heredity from the parent in question. The other parent has also contributed a chromosome in which is located a gene for eye color—possibly for blue. As brown happens to be a dominant color, this blue-eye gene is temporarily lost to view, only to reappear later among some of the child's descendents in the absence of genes for a dominant color. If some mental trait, such as musical ability, should happen to have one of its genes located in the same chromosome, it does not follow that the parent whose gene for eye-color was dominant over that of the other parent should show a similar dominance for musicality. On the contrary, it is a matter of pure chance which parent, if either, should do so. Many genes, like those for color in the blue Andalusian fowl, do not exhibit the phenomenon of dominance to any appreciable extent, but display, instead, a blending effect when combined.

In the case of siblings, the situation is altered. Though each sibling receives a full set of 48 chromosomes (24 from each parent), it is a matter of chance which 24 of the 48 chromosomes per parent will fall to his lot. On the average, a pair of siblings, unless they are duplicate twins, share only half their chromosomes in common; but the number of shared chromosomes may vary, according to the law of normal probability, from 48 to zero. When the number of shared chromosomes is less than 6, the relationship of siblings, genetically speaking, is less close than that of ordinary first cousins.

It is reasonable to suppose that siblings who resemble one another very closely in physical appearance have received an unusually large chance number of chromosomes in common, and that siblings who impress an observer as showing little or no family resemblance have an unusually small overlapping of chromosomes. If the particular chromosomes involved in the traits under scrutiny should happen also to contain genes for certain mental traits, we might expect the siblings who looked most alike to be the most alike psychologically, and those who looked least alike to show unusually large mental differences.

NOMINATION OF CASES

In the investigation, which was carried out in Pasadena, every public school was provided with nomination blanks upon which to note the names of pairs of brothers and pairs of sisters who resembled one another unusually much or unusually little in general appearance. Teachers and others who filled out the blanks were not told that the study would later be concerned with the mental traits of these children, for we wished to avoid as far as possible any bias of selection that might distort results. We asked simply for the names of pairs (not twins) "who look so much alike that a person acquainted with one child but not with the second would, on meeting the second, instantly recognize him as belonging to the same family as the first," and of pairs "who are so different in appearance that a stranger would scarcely believe they belonged to the same family."

All siblings nominated from the junior high school and senior high school grades (7 to 12) were rated for general resemblance on a 9-point scale independently by the two writers. In addition to siblings nominated from the elementary schools, *all* like-sex sibling pairs attending two representative elementary schools were seen and independently rated. The latter group, consisting of 141 pairs, was used for computing the reliability of the ratings, and for determining the proportions of random sibling pairs which display physical resemblance of varying degrees. In this group, as in the group of nominated pairs, children meeting certain criteria to be described were selected for further study.

In order to simplify interpretation, only white English-speaking children of north-European or mixed "American" descent were included in the statistical treatment which follows. Half-siblings were, of course, excluded. It is believed that the results are free from selective influences other than those always attendant upon studies of siblings taken from a school population, and that the correlations can, therefore, be fairly compared with those reported by other investigators.

THE RATINGS

The following scale was used by the writers in making their ratings:

1. Almost as much alike in general appearance as identical twins
2. Very marked family resemblance
3. Marked family resemblance
4. Slight family resemblance
5. No noticeable resemblance
6. Slight difference in type
7. Marked difference in type
8. Very marked difference in type
9. As different as two people of the same race could be

The writers carefully agreed upon the significance which they would assign to the scale, and made several practice ratings before beginning their field work. Their independent ratings of (a) the siblings nominated from the junior and senior high schools, (b) the siblings nominated from the elementary schools, and (c) all like-sex sibling pairs attending two elementary schools (including some from b) gave the following correlations:

	r	*P.E.*$_r$	*N*
a	.71	.04	80
b	.75	.04	67
c	.67	.03	141

The fact that correlations a and b are slightly higher than the third is due to the presence of a larger number of extremely similar or dissimilar pairs among the nominated cases than among the random cases. If correlation c is corrected by the Spearman-Brown formula to give the reliability of the average of two ratings in an unselected group, the result is .80.

Average ratings were distributed in the group of 141 unselected sibling pairs as in Table I.

COLLECTION OF DATA

The original plan of the experimenters was to administer the Stanford-Binet test to the like-appearing siblings of the elementary schools whose average rating of resemblance was 2.0 or better. Only 3.5 per cent of the unselected siblings received so high a rating. As this boundary netted only 17 cases, it was extended to include sib-

TABLE I

RESEMBLANCE RATINGS OF UNSELECTED SIBLING PAIRS

Rating Interval	*Boys*	*Girls*	*Total*	*Percentage Reaching or Exceeding*
1.5-1.99		3	3	2.1
2.0-2.49	4	4	8	7.8
2.5-2.99	5	10	15	18.4
3.0-3.49	15	14	29	39.0
3.5-3.99	23	18	41	68.0
4.0-4.49	15	5	20	82.2
4.5-4.99	5	4	9	88.6
5.0-5.49	4	2	6	92.8
5.5-5.99	3	1	4	95.7
6.0-6.49	1	2	3	97.8
6.5-6.99		1	1	98.5
7.0-7.49		1	1	99.3
7.5-7.99	1		1	100.0

TABLE II*

RESEMBLANCE RATINGS OF ELEMENTARY SCHOOL LIKE-APPEARING PAIRS

Average Rating	*Percentage of Unselected Siblings Reaching or Exceeding*	*Number of Pairs Tested*
2.0 or higher	3.5	17
2.25	7.8	6
2.5	15.6	11
		34

* The sisters in one of the 34 pairs were attending a junior high school, but as both children were under fourteen years of age they were given the Stanford-Binet as well as the Terman Group Test.

lings whose average ratings were distributed as in Table II. The 34 cases of Table II include 11 pairs of brothers and 23 pairs of sisters. In 24 pairs the older child was tested by the junior author and the younger child by the senior author, in 2 cases Miss Natalie Raymond and the senior author each tested one member of a pair, and in 8 cases the senior author tested both members of each pair.

Limitations of time prevented the testing of a comparable group of elementary school unlike-appearing sibling pairs. Four pairs of brothers whose average ratings ranged from 5 to 7 were tested, however, with interesting results.

Terman Group Test scores were obtained for all sibling pairs nominated and rated at the junior and senior high schools. The distribution of average ratings of resemblance in these pairs is shown in Table III. In approximately 60 per cent of the cases the Terman Group Test was administered by the writers. Scores upon this test

TABLE III

RESEMBLANCE RATINGS OF JUNIOR AND SENIOR HIGH SCHOOL NOMINATED PAIRS

Rating Interval	*Boys*	*Girls*	*Total*
1.5-1.99		1	1
2.0-2.49		2	2
2.5-2.99	1	3	4
3.0-3.49	4	6	10
3.5-3.99	12	11	23
4.0-4.49	5	9	14
4.5-4.99	4	6	10
5.0-5.49	2	5	7
5.5-5.99	2		2
6.0-6.49	1	4	5
6.5-6.99	2	1	3
7.0-7.49	3		3
	36	48	84

were obtained for the remaining cases from the school files. The ratings were made before the tests were scored or obtained from the files, and nearly all the tests were scored by a third person who was unfamiliar with the ratings. Four pairs are included in Table III who were rated by only one of the experimenters.

Tables I, II, and III all indicate that the standards employed by the writers in rating the girls were somewhat more lenient than those used in rating the boys, for more girls than boys received high ratings.

TREATMENT OF DATA

1. In the group of 34 elementary school like-appearing siblings the IQ of the older member of each pair was correlated against the IQ of the younger member.

2. In the junior and senior high school group of 84 pairs, the following subdivisions were made:

a. 20 like-appearing pairs whose ages were less than 15-0 (rating 3.5 or better)
b. 23 unlike-appearing pairs whose ages were less than 15-0 (rating less than 3.5)
c. 32 like-appearing pairs of all ages, including those of (*a*) (rating 3.5 or better)
d. 52 unlike-appearing pairs of all ages, including those of (*b*) (rating less than 3.5)
e. 20 cases with highest ratings of resemblance, regardless of age (rating 3.5 or better)
f. 20 cases with lowest ratings of resemblance, regardless of age (rating 5.0 or less)

Terman Group Test IQ's were computed for Groups *a* and *b* in the usual way. In Groups *c* and *d* certain empirical assumptions were made—that the average IQ of the children in age groups above fourteen was equal to that of the children of lesser age, and that it took the same number of points on the test to equal one point of IQ among children above fourteen as it did among children of fourteen. These assumptions are probably only approximately accurate, but give "estimated" IQ's whose values are more reasonable than those yielded by a MA/CA ratio in children over fourteen. As in the case of Group 1, the IQ of the older child was correlated against that of the younger child of each pair.

RESULTS

Table IV summarizes the correlation coefficients between the IQ's of the sibling groups just described.

The IQ's of the children in Groups 1 and 2 had means and standard deviations as shown in Table V, in which the mean chronological ages are also listed.

TABLE IV

CORRELATIONS BETWEEN IQ'S OF LIKE- AND UNLIKE-APPEARING PAIRS

	r	*P.E.*$_r$	*N*
Group 1, elementary school, likes	.45	.09	34
Group 2, junior and senior high school			
a. Likes under 15-0	.78	.06	20
b. Unlikes under 15-0	.31	.13	23
c. Likes of all ages	.67	.07	32
d. Unlikes of all ages	.61	.06	52
e. Pairs most like	.61	.09	20
f. Pairs most unlike	.63	.09	20

TABLE V

MEASURES OF CENTRAL TENDENCY AND DISPERSION OF IQ'S

	Mean IQ	*S.D.*	*Mean CA*	*N*
Group 1 (likes)				
Older of pair	101	14	9-9	34
Younger of pair	106	12	7-5	34
Group 2 (likes)				
Older of pair	101	15	14-4	32
Younger of pair	107	14	12-8	32
Group 2 (unlikes)				
Older of pair	101	12	14-9	52
Younger of pair	106	14	13-1	52

With the exception of correlations *a* and *b* for Group 2, the correlations reported in Table IV are fully in accord with those found by other investigators for groups of siblings of similar grade range measured upon tests similar to those used in this study. Upon individual tests (chiefly the Stanford-Binet), various investigators have reported sibling correlations as follows:

Rensch (11) (1921)	.45
Elderton (2) (1922)	
using Gordon's data	.54
using Drinkwater's data	.38 to .53
Hart (3) (1924)	.40 to .46
Madsen (8) (1924)	.63
Hildreth (4) (1925)	.41
Jones (6) (1928)	.49

Thorndike (15) has reported correlations of .66 and .73 between I.E.R. group test scores (interpreted in terms of age norms) of siblings attending high schools.

Since correlations *a* and *b* for Group 2 (Table IV) are based upon only a small number of cases, and are not checked by any of our other correlations, we are not justified in attributing significance to the difference between them. Correlations *c* and *d*, based upon larger numbers, display a difference that is insignificant in the light of its probable error. In correlations *e* and *f*, what slight difference occurs is in favor of the unlike group! The correlation, .45, between the like pairs of Group 1 is certainly no higher than would be expected on the basis of previous work with siblings tested on the Stanford-Binet. In this connection, it is interesting to compare the differences found between the Stanford-Binet IQ's of four pairs of very unlike-appearing brothers. The average IQ difference for the four pairs was 9.5 as against 13.4 for the 34 like pairs, and 12.0 for a group of siblings differing less than two years in age who were studied by Tallman (14). The present study, then, offers no evidence in support of linkage between intelligence and general physical appearance of siblings.

It is possible that the siblings considered in this study were not selected rigorously enough to show linkage if it exists. To gain a little light on this possibility, we examined the records of four pairs of siblings who resembled one another so closely in appearance that only their differences in size served to distinguish them for the casual observer. The IQ's of these pairs were as follows:

1. Brothers of ten and seven, IQ's 103 and 105
2. Sisters of eleven and ten, IQ's 92 and 91
3. Sisters of eleven and eight, IQ's 128 and 95
4. Sisters of seven and five, IQ's 87 and 116.

Two pairs were very similar in IQ, but the other two pairs showed differences in IQ that were exceeded by only one other pair of Group 1.

Fingerprints were taken by the writers for Cases 1, 2, and 4, but not for Case 3 because of the illness of one of this pair. The patterns are indicated below. The friction ridge patterns of duplicate twins have proved to be so amazingly similar that Newman and others use them for one of the chief criteria in distinguishing between fraternal and duplicate twins. The writers believed that friction ridge patterns might be of great assistance in determining the amount of overlapping in sibling inheritance.

In Case 1, which is really the most striking case of physical resemblance among all the pairs studied, only two pairs of corresponding fingers have the same patterns. Yet the IQ's of these brothers differ by only two points. In Case 2 there is a high degree of correspondence between the finger patterns of the two sisters, and also a close correspondence in their IQ's (difference of only one point).

	Left Digits					*Right Digits*				
	1	2	3	4	5	1	2	3	4	5
Case 1										
Oldest	W.	L.P.	L.P.	W.	L.P.	U.	L.P.	L.P.	W.	U.
Youngest	U.	R.	T.A.	U.	U.	U.	T.A.	U.	U.	U.
Case 2										
Oldest	U.	T.A.	U.	U.	U.	U.	T.A.	U.	U.	U.
Youngest	U.	T.A.	U.	U.	U.	U.	U.	U.	U.	U.
Case 4										
Oldest	U.	U.	U.	U.	U.	U.	R.	U.	U.	U.
Youngest	U.	U.	U.	U.	T.A.	U.	A.	T.A.	U.	U.

But the fact that most of the patterns are ulnar loops deprives this comparison of some of its significance, since the ulnar loop has the highest incidence of all patterns in the general population. In Case 4, which shows the large difference of 29 points of IQ, 7 of the 10 pairs of fingers have the same pattern (though all seven are ulnar loops). While it is possible that palm patterns, which were not taken, would have yielded results of positive significance, the fingerprint comparisons seem to furnish no explanation of the mental resemblance of Cases 1 and 2, nor of the disparity of Case 4.

SUMMARY

The study investigates the possibility that some of the genes involved in mental ability and general physical appearances are linked. A rating scale of physical resemblance was devised for use in selecting cases for mental tests. The average of two ratings on general resemblance had a reliability of .80 in a group of 141 white, English-speaking, like-sex sibling pairs attending elementary schools.

Intelligence tests were administered to a group of 34 elementary school sibling pairs showing strong physical resemblance, and to 84 pairs of junior high school and high school sibling pairs subdivided into groups showing much and little physical resemblance. The resulting correlations do not support the view that siblings who look most alike are most alike psychologically, or that those who look least alike show unusually large mental differences.

Of four elementary school pairs whose physical resemblance was extremely close (within the top 2 per cent of random pairs), two pairs were very similar in IQ, but two pairs showed differences

in IQ that were exceeded by only one other pair of the elementary school series. Fingerprint comparisons obtained in three of these four cases failed to shed light on the causes of IQ resemblance and disparity.

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Six—The Intelligence Quotient of Francis Galton in Childhood

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REPRINTED FROM THE ARTICLE IN *American Journal of Psychology*, 28 (1917), 208-215, BY PERMISSION OF THE AUTHOR AND THE PUBLISHER.

Terman, a distinguished American psychologist, became interested early in his career in problems related to genius. The man of genius has many legends associated with him. One is that he is frequently dull or mediocre as a child. Terman began the investigation of this and other questions. He and his collaborators obtained all the facts that were available concerning the childhood of the world's 300 most eminent men. The first person to be investigated was Sir Francis Galton, one of the world's most brilliant men. This first study of the early IQ of a man of genius is presented below.

The results of the investigation of the 300 eminent men are presented in a book by Cox entitled Early Mental Traits of Three Hundred Geniuses. *Terman has also studied the careers of a large number of living persons chosen because of high intelligence in childhood. The results of these investigations have been presented by Terman and his co-workers in a series entitled* Genetic Studies of Genius *of which the book mentioned above is the one volume.*

The writer does not remember to have seen Francis Galton classed among boy prodigies. Indeed, Galton's main contributions to science were given to the world at so late a date in his life that he is not infrequently mentioned as an illustration of late-maturing genius. *Hereditary Genius* was published in the author's fiftieth year; *Natural Inheritance* in his sixty-eighth year of age.

In the recently published Volume I of Karl Pearson's *Life,* *Letters and Labors of Galton*, there is ample evidence that Galton was a boy of unusual attainments and that he was extraordinarily precocious. The biography in question departs radically from the usual type of biography by presenting documentary evidence regarding the more important events in the life of its subject, and that concerning Galton's childhood is especially full and significant. From the evidence given, one is justified in concluding that between the ages of three and eight years at least, Francis Galton must have had an intelligence quotient not far from double his actual age.

The significance of this will be apparent when we say that after diligent search in several cities and several counties in California—a search including many thousands of children in scope—the highest intelligence quotient we have yet found is 170. The number that we have found going above 150 can be counted on the fingers of one hand.

From early childhood Galton was under the instruction of his sister Adèle, herself a mere child. "She taught him his letters in play, and he could point to them all before he could speak. Adèle had a wonderful power of teaching and gaining attention without fatiguing. She taught herself Latin and Greek that she might teach him. She never had him learn by heart, but made him read his lesson, bit by bit, eight times over, when he could say it. He could repeat much of Scott's *Marmion* and understood it all by the time he was five." (Quoted by Pearson from Elizabeth Ann Galton's *Reminiscences.*)

Pearson further informs us that Francis knew his capital letters by twelve months and both his alphabets by eighteen months; that he could read a little book, *Cobwebs to Catch Flies*, when two and a half years old, and could sign his name before three years. The following letter from his fourth year has been endorsed by his mother, saying that Francis wrote and spelled it entirely himself;

"My
dear
Uncle
We have
got Ducks. I know
a Nest. I mean
to make a
Feast."

The day before his fifth birthday he wrote the following letter to his sister:

"My dear Adele,

I am 4 years old and I can read any English book. I can say all the Latin Substantives and Adjectives and active verbs besides 52 lines of Latin poetry. I can cast up any sum in addition and can multiply by 2, 3, 4, 5, 6, 7, 8, [9], 10, [11].

I can also say the pence table. I read French a little and I know the clock.

Francis Galton,
Febuary 15, 1827."

The only misspelling is in the date. The numbers 9 and 11 are bracketed above, because little Francis, evidently feeling that he had claimed too much, had scratched out one of these numbers with a knife and pasted some paper over the other!

This document should have great interest for those who have worked with mental tests. That Francis at less than five years could read any English book demonstrates beyond any possible doubt that he was as far advanced at this time as the *average* English or American child at nine or ten years. It is an accomplishment which we do not believe is possible to a mental age of less than nine years with any amount of formal instruction. It is certain that our subject's accomplishments did not include merely the ability to pronounce words mechanically, for there is ample evidence from other sources that at this early age he read with understanding.

Again, at this age Francis had learned to do any sum in addition, and had learned all but the hardest part of the multiplication table. This indicates, at least, nine-year intelligence, for we have found that, however old a child and however much school instruction he may have had, the multiplication table is seldom mastered thoroughly much below the nine-year level. Further, his knowledge of the "pence table" indicates an acquaintance with the coins and their values such as children ordinarily do not have before something like eight years.

Besides informing us that Francis had, at this tender age, gotten quite a start in French and Latin, the above letter also tells us that he "knows the clock"; that is, presumably, he was able to tell the time of day by the clock. This performance has been definitely standardized at the mental age of nine to ten years, and it is almost never passed before the mental age of eight years.

The reader may raise the question whether it is safe to accept a child's own statements with regard to the above points. It would not be, of course, if there were no corroborative evidence. The fact that there is such evidence from many sources, and the fact that little Francis was known to be as remarkably conscientious as he was intelligent, justifies us in accepting the above statements without the slightest discount.

The fact that Francis' reading at the age of five years was intelligent and not of the mechanical kind is demonstrated by his ability at that age to offer quotations which would fit a given situation. For example, when he was five years old, a boy friend asked his advice as to what he ought to say in a letter to his father, who, it seems, was in danger of being shot for some political affair. Little Francis replied immediately from Walter Scott:

"And if I live to be a man,
My father's death revenged shall be."

Again at the age of five, he was found holding a group of tormenting boys at arm's length, shouting meanwhile,

"Come one, come all. This rock shall fly
From its firm base, as soon as I."

By six, under the tutelage of Adèle, he had become thoroughly conversant with the Iliad and the Odyssey. At this age, a visitor at the Galton home made Francis weary by cross-questioning him about points in Homer. Finally, the boy replied, "Pray, Mr. Horner, look at the last line in the twelfth book of the Odyssey" and then ran off. The line in question reads, "But why rehearse all this tale, for even yesterday I told it to thee and to thy noble wife in thy house; and it liketh me not twice to tell a plain-told tale."

It seems that Adèle also taught Francis a good deal about entomology, and at six and seven years he was active and persistent in collecting insects and minerals, which he is said to have classified and studied in more than a childish way. It has been shown by Mrs. Burk that collections of an analytical and classificatory type are not common before twelve or thirteen years. Here, again, we find evidence of an intelligence quotient not far from 200.

Pearson quotes the following letter written by a visitor at the Galton home on December 28, 1828:

"The youngest child, Francis, is a prodigy. He is seven next February and reads 'Marmion,' 'The Lady of the Lake,' Cowper's, Pope's and Shakespeare's works for pleasure, and, by reading a page twice over, repeats it by heart. He writes a beautiful hand, is in long division, and has been twice through the Latin Grammar; all taught by Adèle."

At the age of eight, Francis was taken away from home to attend a boarding school. Here he was placed in a high class, although the boys in it ranged up to fifteen years. Since this was a private school attended by children of a superior social class, it is altogether likely that his fourteen- and fifteen-year-old classmates were themselves above the average mental level of that age; hence Francis must by this time have reached a mental level of not far from that which is median for sixteen years.

In his first year at this school, we find Francis writing to his father in these words; "I am very glad that you have left off being a banker, for you will have more time to yourself and better health." This little quotation certainly betokens a degree of filial solicitude by no means common to children of this age. Such altruism does not ordinarily develop so early. The words fit sixteen-year much better than eight-year intelligence.

Francis' interests at the age of ten are indicated by the following letter:

December 30, 1832.

My Dearest Papa:

It is now my pleasure to disclose the most ardent wishes of my heart, which are to extract out of my boundless wealth in compound, money sufficient to make this addition to my unequaled library.

The Hebrew Commonwealth by John	9
A Pastor Advice	2
Hornne's commentaries on the Psalms	4
Paley's Evidence on Christianity	2
Jones Biblical Cyclopedia	10
	27

It is hardly necessary to comment on the above letter as an indication of the boy's mental maturity. It speaks for itself.

Francis' interests, however, were not wholly literary, for at the age of thirteen he gave us "Francis Galton's Aerostatic Project." It seems this was a series of drawings representing a flying machine. It was to work by large, flapping wings with a sort of revolving steam engine, and was supposed to carry five passengers, a pilot, and an engineer.

At the age of fifteen, we find the youth Galton expressing in his letters to his father serious opinions on mind training, the relative value of classics and English, and other matters of educational theory. These opinions were voiced by him again some sixty years later, substantially without change.

At the age of fifteen Francis was admitted to the general hospital at Birmingham as a medical student.

It is well known that, in general, a high correlation obtains between favorable mental traits of all kinds; that, for example, children superior in intelligence also tend to be superior in moral qualities. Francis Galton was no exception to this rule, as indicated by the following letter written by his mother when the boy was only eight years old: "Francis from his earliest age showed highly honorable feelings. His temper, although hasty, brought no resentment, and his little irritations were soon calmed. His open-minded disposition, with great good nature and kindness to those boys younger than himself, made him beloved by all his school fellows. He was very affectionate and even sentimental in his manners. His activity of body could only be equaled by the activity of his mind. He was a boy never known to be idle. His habit was always to be doing something. He showed no vanity at his superiority over other boys, but said it was a shame that their education should have been so neglected."

After Mr. Pearson has given us all the above significant information, it is astonishing to find him commenting upon it as follows: "The letters we have quoted from these early years may appear to the reader to contain little of note. They are, indeed, just what a healthy normal child would write, but it is that very fact that makes them essential human documents and gives them their fundamental interest. . . . Need we attempt to see signs of exceptional ability or to discover foreshadowings of future achievement in these outpourings of healthy childhood? I do not think we can say more than that Francis Galton was a normal child with rather more than average ability" and so on.

Mr. Pearson's error is of a kind which is now coming to be generally recognized by those who work with mental tests; that is, an error due to the failure to take into account the significance of a mental performance *in terms of the mental age to which it corresponds*. Pearson did not know, and the average teacher does not fully appreciate, that a child of four years who is able to do the things characteristic of a child of seven or eight years is a genius of the first order. It is hard to get people to understand that what a child is able to do has no significance unless we take age into account.

The opposite error is no less common; that is, for a mentally retarded child in a grade far below his age to be considered perfectly normal and average in intelligence. One was only recently described as "slow to learn." The child in question was twelve years old and in the first grade, and we suggested to the teacher that in all probability the child was feeble-minded. We were met, however, with the most positive assurance that the little girl in question could not possibly be feeble-minded, that she was actually learning the work of the first grade, and that her normal mentality was shown by her motherly interest in her little six-year-old classmates. Without arguing the matter further, we urged the teacher to bring the child for a Binet test, with the result that she was found to have a mental age of a little less than six years by the Stanford Revision. This child had been in school several years and had had every opportunity to learn, except the advantage of endowment. Experience has taught us that such a subject will never reach the mental level of seven years, however long she may live.

This teacher's error may seem to some almost incredible. In reality it is an error of about the same degree as that made by Mr. Pearson, though in the opposite direction.

VII–EMOTION

One—A Theory of the Native Emotions

JOHN B. WATSON &
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It is useful to illustrate the errors of science as well as its successes. Watson and Morgan, on the basis of general observation, described three patterns of infant response, which they called the native emotions. But Watson and Morgan did not determine whether or not different observers would agree with them in designating the responses of the infant nor did they describe what the infant does in situations other than the three they employed.

The student will gain some knowledge of the halting way in which child psychology has progressed by reading Watson and Morgan's theory of the native emotions in conjunction with the two subsequent sections, which are criticisms of it. He should be warned that Watson and Morgan's theory has been very generally abandoned.

After observing a large number of infants, especially during the first months of life, we suggest the following group of emotional reactions as belonging to the original and fundamental nature of man: *fear*, *rage*, and *love*, (using love in approximately the same sense that Freud uses sex). We use these terms which are current in psychology with a good deal of hesitation. The reader is asked to find nothing in them which is not fully statable in terms of situation and response. Indeed we should be willing to call them original reaction states, X, Y, and Z. They are far more easily observable in animals than in infants. While we do not claim that this list is complete, we do claim that our own observation of the first few months of infancy has not yielded any larger number.

FEAR

What stimulus apart from all training will call out fear responses; what are these responses; and how early may they be called out? The principal situations which call out fear responses are as follows: (1) To suddenly remove from the infant all means of support, as when one drops it from the hands to be caught by an assistant. (In the experiment the child is held over a bed upon which has been placed a soft feather pillow.) (2) By loud sounds. (3) Occasionally when an infant is just falling to sleep the sudden pulling of the blanket upon which it is lying will produce the fear responses. (4) Finally, again when the child has just fallen to sleep or is just ready to awaken, a sudden push or a slight shake is an adequate stimulus. (2) and (4) above may be looked upon as belonging under (1). The responses are a sudden catching of the breath, clutching randomly with the hands (the grasping reflex invariably appearing when the child is dropped), blinking of the eyelids, puckering of the lips, then crying; in older children possibly flight and hiding (not yet observed by us as "original" reactions). In regard to the age at which fear responses first appear as we can state with some sureness that with few exceptions the above-mentioned group of reactions appear at birth. It is often stated that children are instinctively afraid in the dark. While we shall advance our opinion with the greatest caution, we have not so far been able to gather any evidence to this effect. When such reactions to darkness appear they are due to other causes; darkness comes to be associated with absence of customary stimulation, with noises, and so on. (They should be looked upon as conditioned fear reactions.) From time immemorial children have been "scared" in the dark, either unintentionally or as a means of controlling them.

RAGE

In a similar way the question arises as to what is the original situation which brings out the activities seen in rage. Observations seem to show that the *hampering of the infant's movements* is the factor which apart from all training brings out the movements characterized as rage. If the face or head is held, crying results, quickly followed by screaming. The body stiffens and fairly well coordinated slashing or striking movements of the hands and arms result; the feet and legs are drawn up and down; the breath is held until the child's face is flushed. In older children the slashing movements of the arms and legs are better coordinated and appear as kicking, slapping, biting, pushing, and the like. These reactions continue until the irritating situation is removed, and sometimes do not cease then. Almost any child from birth can be thrown into a rage if its arms are held tightly to its sides: oftentimes even if the elbow joint is clamped tightly between the fingers the responses appear; at times just the placing of the head between cotton pads will produce them. Even the best natured child will show rage if its nose is held for a few seconds.

LOVE

The original situation which calls out the observable love responses seems to be the stroking or manipulation of some erogenous zone, tickling, shaking, gentle rocking, patting, turning upon the stomach across the attendant's knee, and so on. The response varies —if the infant is crying, crying ceases, a smile may begin, attempts at gurgling, cooing, and finally, in slightly older children, the extension of the arms which we should class as the forerunner of clasping in the narrowed sex act in coitus. The smile and the laugh which Freud connects with the release of repression (we are not denying in the case of adults that this may not be true) we should thus class as original reaction tendencies intimately connected from infancy with the stimulation of the erogenous zones. By original nature the child is not *polymorpheus perverse,* nor addicted to playing with urine, fecal matters, or (originally) even with the anus or sex organs. Habits (conditioned reflexes) are rapidly set up in connection with these objects at a *very* early age and they may when not looked after sadly warp the child. Jung is quite right in giving up Freud's conception of the original nature of the child, but at such an early age many habits of perversity arise that one not observing children from birth might very well suppose that the acts in question belonged to its original nature. From birth the child will put a finger or a part of the hand in its mouth (native reflex connected with feeding). There is no such original tendency to bring the hand to the sex organs. The earliest well-marked responses of this kind we have seen were observed in a baby girl approximately one year old. She was in her bath playing with a small box. In her efforts to get it her hand slipped and came in contact with the vagina and entered it. A characteristic smile overspread her face and her efforts to recapture the box ceased.

But at this age the child had formed varied habits of manipulation. Our contention here is that her own manipulation of the erogenous zone was brought about by the formation of a habit. That such a conditioned reflex or habit might arise at an earlier age is beyond question.

Two—The Interpretation of Emotional Responses in Infants

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This study shows how difficult it is for observers to know what is causing them to perceive as they do. Sherman's observers thought they were observing the infants' behavior, but the research results indicate that they were judging the responses primarily in terms of the stimuli.

This paper deals with one phase of the work on the differentiation of emotional responses in infants and is concerned with the ability of various observers to name and differentiate the reactions of infants below twelve days of age to stimuli which presumably elicit distinctive emotional responses.

The specific problems were whether the observers could agree as to the emotional characteristics of these responses, whether the judgments were based upon observable differences in the infants' responses or upon the knowledge of the stimuli employed, and whether the previous training and experience of the observers were factors in the judgments made.

PROCEDURE

Four types of stimuli were employed: hunger, dropping suddenly, restraint of the head and face, and sticking with a needle. For the hunger response the infants were observed about 15 minutes past the proper feeding time (the infants had not been fed). For the second type of response the infants were dropped suddenly for a distance of from two to three feet toward the table. For restraint, the head and face were held down on the table with fairly firm pressure. For the fourth reaction the infants were given six successive stimuli on the cheek with a needle. The various stimuli were given at different times of the day so as to obviate any possible "running over" of the stimuli or of the reactions.

The judgments of the various observers were obtained under the following conditions. (a) A group of observers were presented with motion picture views of the stimulating circumstances and the ensuing responses, and the observers were asked to write down the name of the emotions. In the case of the hunger reaction the observers were informed of the nature of the stimulating circumstances. (b) With a second group of subjects the stimulating conditions were deleted from the pictures, and the observers were asked to name the emotion when only the responses were observed. They were also asked to give a judgment as to the nature of the stimuli used to bring out the various reactions. (c) With a third group of observers the stimuli and responses were transposed in the film, and the subjects were required to name the emotional character of each response when it was presented with a stimulus different from that which aroused it. (d) The infants were stimulated behind a screen, which was immediately removed, and the subjects were required to name the emotion on the basis of direct observation of the infants' responses. This method of presentation gave a number of additional sensory data, such as flushing of the face, and the character of the cry.

For the motion picture views the infants were photographed in a light room, the light being just strong enough for the photography but not of sufficient strength to cause any perceptible reaction in the infants. The infants were undressed except for the abdominal bandage and were placed upon a well-padded table. For the hunger response the infants were photographed 15 minutes past the proper feeding period, and no attempt was made to elicit any other type of response within five hours. The method of presentation of the film was as follows. The film contained about 15 feet of views of the infant in its normal resting state, followed by the stimulating condition, the reaction of the infant, and an interval of opaque leader during which the observers gave their judgments in writing. The rest of the film then followed in the same order. In the experiments with the observers who were not shown the stimuli the parts of the film showing the stimulating circumstances were replaced by opaque leader film. Since the hunger stimulus could not be shown on the film, about 15 feet of opaque leader was used, the experimenter explaining that it denoted the missing of a meal.

The observers of the motion picture views were given small slips of paper on which were typed the numbers 1 to 4 under each of which were written the words Emotion and Stimulus. After each reaction of the infant was shown the room was lighted and the observers asked to write the name of the emotion and the probable stimulus for each reaction when the stimulating conditions were not shown. After all of the film was shown the observers were asked to write on a separate sheet of paper the reasons for their differentiation between the various reactions of the infants.

SUBJECTS

The infants employed were normal and full-term, and only those whose nursing charts showed them to be progressing normally were used. For the motion picture photography two infants were used, R., age seventy-four hours, and M., one hundred and forty-five hours of age. For the actual presentation in the nursery the infants were picked at random, and only one type of stimulus was given each infant when shown to a group of observers. The hunger reaction was not attempted when the infants were shown to the observers, because of the conditions in the nursery and the confusion which the inclusion of this reaction would create.

RESULTS

1. The judgments of a group of graduate students in psychology to the motion picture views when the stimuli were not shown are given in Table I. The list of judgments are tabulated in the first vertical column, and the numerical data indicate the number of each of these judgments that were given for each of the stimulating conditions. For example, the judgment of anger was most frequently given; 11 times to the hunger reaction, 14 times to the reaction following dropping, 13 times to restraint, and 8 times to the response to sticking with a needle.

It is seen, then, that although a fairly large percentage of the students named anger to the reaction following restraint, a greater number named anger to the reaction following "dropping," and an almost equal number gave the same name to the reaction following the missing of a meal. A similar dispersion is seen in naming the emotion in the other reactions; 9 observers named fear to the reaction following sticking with a needle, 7 to that following the delay in feeding, and only 5 students named fear to the reaction following "dropping." To the reaction following restraint 8 different "emotions" were named, 7 were named to the reaction following the delay in feeding, to sticking with a needle 8 "emotions" were named, and 8 "emotions" to that following restraint.

TABLE I

JUDGMENTS OF GRADUATE STUDENTS IN PSYCHOLOGY TO MOTION PICTURES OF THE REACTIONS OF INFANTS WHEN THE STIMULI WERE NOT SHOWN

Judgments	*Stimuli Calling Out the Reactions*				Total
	"Hunger"	Dropping	Restraint	Sticking with needle	
	Number of students naming the emotion				
Hunger	7	6	2	2	17
Anger	11	14	13	8	46
Fear	7	5	5	9	26
Pain	3	3	4	2	12
Grief	1	1		1	3
Hurt				1	1
Rage	2	1	3	1	7
Discomfort			1		1
Sleepy			1		1
None				1	1
Consternation	1	1			2
Nausea		1			1
Physical discomfort			1		1
Total*	32	32	30	25	119

* The totals are not the same because not all observers made judgments to every reaction shown.

It is seen that the graduate students in psychology showed no agreement in naming the emotions shown by the infants' reactions when the stimuli for those reactions were not included in the motion picture presentation. Out of a total of 119 answers anger was the most frequent named, 46 judgments of "anger" being made. No definite relationship is noted between the answers given by the students and those which are generally expected of observers who are shown the reactions of infants to such stimuli as were employed in this experiment. The judgments of hunger, anger, fear, and pain included 101 of the total of 119 answers. Although this may at first indicate that the judgments made corresponded with the answers generally expected two factors must be considered in explaining the frequency of these answers. First, as was pointed out above, there was no correspondence between the emotion named and the answer expected, the emotions named being dispersed widely. Second, in the vocabulary of the average student the names of hunger, anger, fear, and pain stand out prominently, and it may be that the frequency of these answers is due to this factor of vocabulary.

2. In order to study the influence of more sense data than can be shown in the motion picture views, a group of third-year medical students were shown the reactions of the infants in the nursery. In this way the observers noted in addition the crying, the rush of blood, and so on, which were not as clearly evident in the motion pictures.

Several infants were picked at random, all of ages between one hundred and twelve and one hundred and sixty hours. The same stimuli and method of experimentation were used as in the photography experiments. About 50 medical students who were finishing their third year in the medical school were the observers. During the course of instruction in neuropsychiatry in the clinic the various emotions were discussed especially with emphasis upon their differentiation according to psychological criteria. The students were not told beforehand, however, that they were to be required to observe the emotional responses of infants.

The method of showing the reactions of the infants to this group of students was as follows. The students were taken in small groups to the nursery and grouped behind a screen which hid the experimenter and the infant. Immediately after the stimulus was given the screen was pulled aside and the students observed the reaction until the infant was quiet again. The screen was then replaced and another infant stimulated for another type of response and the students again observed the reaction. They wrote the name of the emotion and the probable cause for the reaction during the periods between the various reactions.

Table II lists the responses of the medical students to the various reactions of the infants. It should be noted that only three types of stimuli were given since the method of experimentation when the observers actually saw the infants made it impossible to show the hunger reaction to the students in the nursery.

It is seen that eight different "emotions" were named to the three reactions shown the students. The most frequently named was "colic," then in order of frequency "anger," "fear," "organic brain emotion," "awakened from sleep," "hunger," and so on. To the reaction following dropping of the infant there were 11 judgments of "awakened from sleep," 11 of "colic," 7 of "anger," and 6 of "hunger." A similar variety of answers was given to the reactions of the infant following restraint and sticking with a needle. The greatest confusion was evident in the students' attempt to differentiate the reaction following dropping from that following restraint.

No agreement is noted in the judgments of the medical students. The answers to the three reactions of the infants show a wide dispersion, and show no definite relation to names of the emotions generally expected as a result of the stimuli given. The medical students who were shown the infants in the nursery and who had all the data possible did "no better" than the graduate students in psychology who were shown the motion pictures.

TABLE II

JUDGMENTS OF THE MEDICAL STUDENTS TO ACTUAL OBSERVATION WHEN THE STIMULI WERE NOT SHOWN

Emotion Named	*Stimuli Calling Out the Reactions*			
	DROPPING	RESTRAINT	STICKING WITH NEEDLE	TOTAL
	Number of students making the response			
Hunger	6	3	3	12
Pain	3	1	6	10
Fear	2	11		13
Anger	7	3	6	16
Colic	11	6	4	21
Awakened from sleep	11			11
Bandage tight	1	3	2	6
Organic brain emotion	1	7	3	11
Total*	42	34	24	100

* The totals are not the same because not all observers made judgments to every reaction shown.

The comparison of Tables I and II shows an interesting difference in the types of responses given by graduate students in psychology and by medical students and this fact indicates that many of the answers were due to differences in the interests and attitudes of the two groups of students. It may be repeated that both groups presumably knew of the work of previous investigators of the emotional development of infants, although the graduate students in psychology were more familiar with the subject matter of psychology. The graduate students in psychology named 13 "emotions" while the medical students named 9. The graduate students named anger, fear, and hunger most often while the medical students named colic, anger, fear, and awakened from sleep most frequently. The difference between the two groups in naming the emotion and in stating the probable cause of the reactions indicates that the particular interests of the individuals were often the deciding factor in their decisions.

3. Because of the lack of agreement of the graduate students in psychology and of the medical students in naming the emotions when the reactions of the infants were shown without presenting the stimuli, a group of graduate students were shown the motion pictures of the reactions together with the stimuli calling out those reactions. This group consisted of graduate students and instructors in psychology. The general observers were told beforehand about the nature of the stimulus to each of the reactions which were to be shown. Each part of the film was shown in the same manner and order that it was shown to the previous group of graduate students except that the stimuli were included in this demonstration. For the stimulus to the first reaction the opaque leader film was explained as denoting a period of 15 minutes past the proper feeding period, the infant not having been fed. This group was also requested to observe carefully the various manifestations of the infants' reactions, such as the arm, leg, and body movements, facial expression, duration and character of the cry, and so on. When the film was finished they were asked to write the reasons for their naming and differentiating the emotional responses.

Table III shows the responses of this group of observers. Twenty-four different names were given, the greatest number being anger, fear, pain, hunger, and discomfort in the order of frequency. To the "hunger" reaction 13 different "emotions" were named, the most frequent being anger. To the reaction following dropping of the infant 10 different "emotions" were named, the most frequent being fear, and to the reaction following restraint 13 names were given, the most frequent being anger. Of the 40 observers 27 answered "fear" to the dropping, 24 named "anger" to the restraint reaction, 13 answered "pain" to the sticking with a needle, and 7 named "hunger" to the reaction of the infant when it missed a meal.

The analysis given by this group of students for naming the emotions and differentiating between the various reactions of the infants is of significance in accounting for the answers. Eighteen of the observers stated that they named the various emotions as they did because they were either directly influenced by seeing the stimuli being given or because they were influenced by previous work in which it was found that certain stimuli give rise to certain definite emotional responses. Five students stated "I named anger, fear, and so on because I knew that those emotions would result from the stimuli given." Three observers stated "I named the emotions because I would feel the same if those stimuli were given to me." Several stated "the child looked angry," and several that the child appeared afraid.

TABLE III

JUDGMENTS OF GRADUATE STUDENTS IN PSYCHOLOGY TO MOVING PICTURE VIEWS OF THE INFANTS WHEN THE STIMULI WERE SHOWN

Answers	*Stimuli Calling Out the Reactions*				*Total*
	DELAY IN FEEDING	DROPPING	RESTRAINT	STICKING WITH NEEDLE	
	Number of students naming the emotion				
Fear		27	4	7	38
Mad			1		1
Discontented	2		1		3
Rage			1		1
Pain	2	2	1	13	18
Negative emotion	1	1	1	1	4
Hunger	7				7
Anger	14	4	24	13	55
Discomfort	6				6
Irritation	1		1		2
Excitement		1			1
Anger or pain	1		1	3	5
Disgusted or weary	1				1
Pain with fear		1			1
Anger with fear		4	1	1	6
Rage	1	1	3		5
Surprise		1			1
Resistance to restraint			1		1
Anxiety	1			1	1
Hate					1
Restiveness	1				1
Repulsion				1	1
Suffocating			1		1
No emotion	2				2
Emotion doubtful		1	1	1	3
Total*	40	43	42	41	166

* The totals are not the same because not all observers made judgments to every reaction shown.

Comparison of Tables I and III indicates the possible influences of a knowledge of the stimuli in the ability of the students to differentiate the emotional responses of the infants, since both groups of students are comparable in regard to psychological knowledge, interests, attitudes, and the like. It is seen that of the students who did not see the stimuli 15 per cent of those who responded to the reaction of the infant after dropping named "fear," while in the group who saw the stimuli 64 per cent of the students who made a decision as to the same reaction named "fear." To the reaction following restraint about 45 per cent of the first group named "anger" while 60 per cent of the second group gave a judgment "anger" to the reaction.

4. The previous experiments had indicated that when the observers knew the stimulus causing the reactions of the infants they showed much more agreement in their differentiation of the emotional responses than the groups of observers who were not shown the stimuli. For that reason it was decided to show the motion pictures to two groups of medical students allowing them to see the stimuli, but with the stimuli transposed.

TABLE IV

RESPONSES OF MEDICAL STUDENTS TO MOTION PICTURE VIEWS WHEN THE STIMULI WERE SHOWN BUT TRANSPOSED

Answers	*Stimuli Calling Out the Reaction*				*Total*
	"HUNGER"	DROPPING	RESTRAINT	STICKING WITH NEEDLE	
	Stimuli Shown to Students				
	Sticking with pin	Restraint	Dropping	"Hunger"	
Fear		4	10		14
Pain	15	3			18
Anger	6	13	7		26
Discomfort		2		3	5
Hunger				17	17
Pain or anger		2	1		3
Intestinal colic				1	1
Terror	2		4		6
Gastric pain				2	2
Pain or fear	1	1	2		4
Irritation	1	3			4
Grief		1			1
Surprise			1		1
Resistance		1			1
Total *	25	30	25	23	103

* The totals are not the same because not all observers made judgments to every reaction shown.

The procedure in showing the film was essentially similar to that used with the graduate students in psychology, that is, the film was shown in separate sections, an interval of about one minute being allowed for the observers to write the name of the emotion. The difference in the film was as follows: The order of stimuli was the reverse of that of the original experiments, that is, the pain stimulus was shown preceding the hunger reaction, the restraint stimulus preceded the "fear" reaction, dropping the infant preceded the "anger" reaction, and the leader portion of the film preceded the "pain" reaction. The students were instructed to note carefully the activity of the infants, such as movements of the arms, legs, and body, movements of the head, duration and apparent intensity of the crying, manner of breathing, and so on, and to give their true opinion of the emotion manifested by the infant upon the basis of objective signs.

Table IV shows a relationship between the stimulus shown and the emotions named. For example, 15 out of 25 students, or 60 per cent, named "pain" to the first reaction which was preceded by the stimulus of sticking with a needle. Thirteen out of 29 students named "anger" to the second reaction, restraint being the stimulus shown. Forty per cent named "fear" to the third reaction, an "anger" reaction preceded by the stimulus of dropping. Seventeen out of 23, or about 74 per cent named "hunger" to the reaction which was presumably pain, the stimulus shown being the leader film denoting a missed meal.

It is seen that the most frequent name of the emotion given by both groups of students was governed not by the actual reaction of the infant but by the stimulus shown preceding that reaction.

SUMMARY

In differentiating and naming the reactions of infants below twelve days of age to stimuli presumably eliciting a particular and definite emotional response, a considerable lack of agreement was apparent in undergraduate and graduate students in psychology and medical students, not only when shown motion picture views of the infants, but also upon actual observation of the infants in the nursery. For the four possible types of reactions shown, namely, hunger, fear, anger, and pain, students in psychology named from 12 to 25 different emotions, when shown motion pictures. When medical students were shown three possible types of reactions in the nursery, namely, fear, anger, and pain, medical students named eight different emotions. The graduate students in psychology had no better success in naming the emotions than the medical students. The difference in the number of emotions named by students in psychology and medical students may possibly be due to the difference in the vocabulary of the two groups. Apparently a knowledge of psychology and of the previous work on the emotions of infants did not aid the graduate students in differentiating between the various emotional reactions. The emotions named by students of psychology differed from those named by the medical students to some extent. This fact is significant in indicating that the emotion designated for a certain reaction depends to some extent upon the interests and attitudes of the observers.

When the stimuli causing the reactions of the infants were shown to the observers the degree of "success" in naming and differentiating between the various emotions was much greater than when the stimuli were not shown. This fact indicates that the knowledge of the stimuli used was an important factor in the observers' decisions.

The reasons given by the two groups of observers for their differentiation between the various emotional reactions indicated that for the most part the differentiation was strongly influenced by the knowledge of the stimulus. This was shown by the fact that the majority of graduate students who were successful in making the differentiation stated that the knowledge of the stimulus influenced them in their decisions or that they knew that no other emotional response would result from a certain stimulus.

The dependence upon the stimuli in naming and differentiating the emotions is clearly brought out by the responses of the group of medical students when the stimuli were transposed. The results indicate that the stimulus preceding the reaction shown was usually the deciding factor in the name given to the reaction.

Three—Infant Reaction to Restraint

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This paper not only extends the criticism of Watson and Morgan's theory, but attempts to present, in one area, an alternative view.

Although the infant in the new born stage may be rightly thought of as having specific unlearned reactions, such as crying, blinking, and sucking, as he grows older his behavior can be understood only in relation to his experience. His interest in the human face is not an attraction to faces per se *but represents his reaction to the agency that ministers to his wants. A face covered by a beard or a veil if it alleviated his needs, would call forth the same smiling and cooing as does the American mother. His fears, likewise, are relative to his experience. If he is reared in a typical American home, he will be afraid of a veiled Mohammedan or a bearded Sikh. But we may be sure that the Mohammedan and Sikh babies develop no such fears. The child of nine to twelve months is afraid of no absolute stimulus but is afraid of unfamiliar stimuli.*

In the field of reaction to restraint, the paper that follows attempts to show that the same sort of principle prevails.

WATSON'S THEORY

Watson's theory of three native emotions, each with its specific stimuli, is well known. Its neatness, its simplicity, its finality make it very satisfying. It has a brevity and conciseness which makes it as delightful as the statement that there are but six continents and three races, and it suffers from some of the same deficiencies.

The views of Watson concerning the emotions are often spoken of as if they constitute a closely knit structure. In point of fact, this is not the case. They may be subdivided into numerous theories, any one of which may be modified without greatly changing the remainder. If other native emotions were added to the list, or if some were subtracted, the general theoretical structure would not be much affected, just as one can add or subtract a continent or a race without altering one's general conception of the world or of the human race.

In this paper, I shall discuss chiefly but one of Watson's hypothesized native emotions; namely, the rage reaction in response to restraint, but I hope that the treatment of this one response will show what is wrong with his scheme, and what will be wrong with any scheme of native emotions or native reaction tendencies which is based on the same principles.

It will be fruitful, first, to sketch the history of Watson's theory. That restraint of movement causes a decided reaction which may be called rage was first stated in 1917, in an article (29) entitled "Psychological Experimentation and Emotional Reactions." The theory is usually attributed to Watson, although the original article was written jointly by Watson and Morgan. While Morgan assisted in the experimentation, he was not associated with the dissemination of the doctrine, and departed from it in later publications. However, for Watson, the theory of the native emotions which was first enunciated in the article cited above was not a view lightly held and easily relinquished. He presented it on many subsequent occasions. It appeared in a volume (15) entitled *Suggestions of Modern Science Concerning Education,* published in 1917 and republished in 1921; in the *Psychological Review,* 1919 (25); in *Psychology from the Standpoint of a Behaviorist,* of which the first edition appeared in 1919 (24) and the second in 1924; in the *Scientific Monthly,* 1921 (30); in the proceedings of the Annual Meeting of the International Kindergarten Workers, 1921 (26); in *Behaviorism,* 1924 (28); and, in later editions, in *The Psychologies of 1925,* (19), and in *The Psychological Care of Infant and Child,* 1928 (27). The wording was practically unchanged throughout. In addition to its presentation in these more scientific works, it also found a place in popular journals. In view of this wide publication by a man who was very much in the public eye at the time, it is not surprising that Watson's theory became a matter of common knowledge nor is it surprising that it was incorporated into many textbooks in psychology and was accepted uncritically for at least a decade.

Watson's experimental contributions to the topic of native reactions may be said to have ended with his first article on the subject; that is, in 1917. All later publications were essentially restatements of his first findings, with no new factual contributions concerning infant emotional patterns. His research was interrupted by his participation in the work of the army during the World War and was ended by his departure from Johns Hopkins in 1920. In spite of his extensive writings, Watson never presented in detail the results of his infant research. He spoke of experimentation upon several hundred infants, but his reports are simply generalized statements of his observations. This form of presentation gives an impression of stereotypy of infant reaction which he probably did not intend to convey.

The stimuli which Watson employed to restrain the infant were numerous. They included holding the head of the infant between the experimenter's hands or between pads of cotton, holding the nose, pressing the arms to the sides, and holding the legs tightly together. Watson repeatedly stated that the essential stimulus to rage was the "hampering of the infant's movements." He claimed that in no case was the pressure which was exerted sufficient to cause real pain, although this is a point concerning which only the baby could have had immediate knowledge. It is interesting to note the behaviorist taking such liberties with the infantile consciousness.

FINDINGS OF OTHER INVESTIGATORS

After Watson, the next experimenters who made use of similar stimuli administered to the newborn infant were Sherman and Sherman in 1925 (21). These investigators applied pressure to the chins of 83 infants. They found that diffuse arm reactions occurred; but the Shermans were more interested in the fact that in older infants the movements were better coordinated than they were in the fact that the reactions, as described by them, were somewhat similar to those which had been reported by Watson.

The later reports of Mandel Sherman (20) are widely known and frequently are cited in partial refutation of Watson's findings. Sherman, in a part of his experiments, exhibited to judges babies which had been stimulated in ways unknown to the judges. He also exhibited movies of infants reacting to stimuli which had been deleted from the films. One of his forms of stimulation consisted in holding the infant's head on the table "with fairly firm pressure." The point which was emphasized by Sherman was the fact that the observers did not designate with much correctness the causes of the reactions which they saw. This, of course, tells as much about the observers as it does about the infants. We do not know whether the reaction patterns of the infants or the perceptual patterns of the observers were loosely organized. However, when the infants were restrained, the observers almost invariably described the reaction as being of a vigorous character, and reported it as anger, fear, pain or colic so that it can be gathered that Sherman's restraint, like Watson's, resulted in pronounced reactions.

Next comes the monograph of Pratt, Nelson, and Sun (18). A section of this publication deals with reaction to restraint. In one experiment the investigators "held the (infant's) arms pressed firmly against the body and held them there against whatever energy the infant would exert." However, in a tabular statement of the ensuing reactions one finds that sometimes the "arms immediately flexed to the original posture" and that sometimes the "arms did not remain at the sides of the body." One must conclude either that the infant occasionally overpowered Pratt, Nelson, and Sun, or that these remarks refer only to what happened after the child was released. If that is the case, then the experimenters failed to state what happened while the infants were being held.

The only condition under which a sufficiently large number of subjects were stimulated by Pratt, Nelson, and Sun was the state described as "asleep and dry." Watson, on the other hand, worked only with wide awake babies, and it is likely that he did not intend his observations to apply to sleeping children.

In another part of their work, Pratt and his collaborators held the infant's nose between the thumb and forefinger with gentle pressure in such a way as to close off the nostrils. In some experiments this was done for 6 seconds, in others the stimulation was continued for 15 seconds. However, if the infant cried, the stimulation was immediately discontinued. In their typical style, these investigators report their results as follows: To 234 experiments on infants who were asleep and dry, there occurred 197 movements of the extremities, 134 of drawing back of the head, and 70 of arching of the spine. These experiments are often cited as being somewhat contrary to the theory of Watson. If, however, it be conceded that Pratt, Nelson, and Sun probably stimulated the subjects less vigorously than did Watson, if it is borne in mind that the condition of the infant when it was stimulated was different in the two investigations, if it is recalled that the duration of the stimulation was not the same, and finally, if one is reminded of the different manner in which the results were reported, it will be seen that there cannot be a contradiction between the two, since the second study scarcely in any sense was a repetition of Watson's. The most reasonable conclusion from Pratt, Nelson, and Sun's work would seem to be that less severe stimuli cause less vigorous reactions.

Taylor's research (23) comes nearer to duplicating Watson's conditions for rage, but in this experiment, too, many of the infants were asleep. Furthermore, the stimulations, which consisted in cutting off inhalation by holding the nose between the experimenter's thumb and forefinger, or in holding the arms at the sides, lasted for only 20 seconds, whereas Watson, in personal correspondence with Taylor after Taylor's experiment was completed, suggested that a duration of one minute should have been used. In Taylor's investigation each of 40 infants was stimulated once by each of the two methods listed above. To nose stimulation, 6 subjects stiffened the body, 11 slashed with the arms, 30 abducted the head, and 21 arched the back. To arm stimulation, 15 stiffened the body and 8 gave vigorous limb reactions. While Taylor concluded that he did not observe Watson's rage pattern, the correctness of this conclusion hinges upon what one means by a pattern. The responses which Taylor observed most frequently were a part of Watson's "pattern" but the pattern did not occur in its entirety at each stimulation.

I wish to report next some experiments which Mrs. Dennis and I performed upon a set of nonidentical twins.* Other researches upon these subjects have been described elsewhere (7, 8, 9, 11), but the following observations have not been previously reported. When the infants were about two months old, they were presented with certain stimuli and their responses to these were recorded by motion picture camera. A series of stimuli was presented each day for 18 days. The stimulations included holding the head between the experimenter's hands, so that it could not be moved, and pressing the nose of the subject with the experimenter's forefinger. The amount of pressure involved in these two forms of stimulation was not measured objectively, but I can say that it was vigorous, since it was desired to obtain some responses to each stimulation.

In any form of experimentation one expects some variation in response from stimulation to stimulation, and this variation Watson's reports tended to ignore. In our records, the variability of the response is marked. Nevertheless, in approximately two-thirds of the stimulations, the movie records show behavior which may be characterized as struggling, thrashing about, or crying. This is not very different from Watson's so-called rage pattern.

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* These are the same subjects referred to on pages 104-131.

But we not only stimulated the infants by holding the nose and by restraining the arms, but also by strong taste stimuli—a saturated salt solution, a very bitter quinine solution, and by dilute citric acid. So far as we were able to determine, the limb reactions to these stimuli were not distinguishable from those which followed the stimulation by restraint. Let me call them by the neutral title of restlessness and crying. It is my belief that many strong and persistent stimuli will cause roughly the same sort of infant reaction.

Was Watson correct in assuming that it was *restraint of movement* which caused the restlessness and crying which he observed or was it caused by intense stimulation? Watson was not a gentle experimenter as anyone will attest who has seen Watson's research film which portrays his methods of infant investigation. He did not merely hold the infant's head and arms; he exerted pressure upon them. Pratt, Nelson, and Sun and Taylor, who merely restrained the movements of their subjects did not obtain such vigorous responses as did Watson.

FETAL RESPONSES

In addition to the observations of the experimenters just mentioned, several other considerations suggest that restraint of movement of itself does not cause restlessness and crying. One of these considerations arises from the situation of the infant in utero. Here, if anywhere, is to be found a profound hampering of movements. The head, the arms, the legs, the trunk, all are restrained. There is no free space in which to move. The arms cannot be extended, the feet meet resistance in kicking, the head cannot be lifted from the chest. Yet the evidence indicates that the mature fetus moves only occasionally. From Watson's theory, one would expect the unborn child to be in a permanent state of violent rage. There is no indication that this is the case, and no indication that the prenatal responses which do occur are the result of restraint. Watson seems never to have faced the question as to why the infant immediately after birth should react emotionally to a condition very similar to that in which it exists for several months in relative quiescence.

If we consider the effective element in Watson's stimulation to have been the intensity of the stimulation rather than its hampering character, no such contradictions in interpretation are met. Intrauterine pressure is not great, and, since the fetus is in a fluid medium, the pressure is approximately equal upon all parts of the body. Each part is subjected only to a very gentle pressure.

SWADDLING AND CRADLING

No psychological theory ever revealed so clearly the need of psychology for an historical and cultural perspective as did Watson's hypothesis of a negative reaction to restraint in early infancy. Had Watson appeared upon the scene of Western civilization prior to 1750, he could not have formulated such a view, nor, in his own time, could he have done so if he had considered the methods of infant care prevalent before the modern era. The theory should have been forestalled by a knowledge of the widespread practice of swaddling and binding the infant. An adequate history of swaddling, so far as I can ascertain, has not been written. We know that swaddling clothes were employed by the Jews, the Greeks, and the Romans before the Christian era, and were used throughout the medieval period in all European countries. Swaddling did not begin to disappear in England until 1750, following a severe criticism of its detrimental effect upon infant health by an influential physician, Cadogan (3). Swaddling is practiced by the Lapps of today. I am told that it persists in some Italian peasant districts, and I have no doubt it is to be found in other parts of Europe as well. The Albanian custom is known through the study of Danzinger and Frankl (6).

In its most extreme form, swaddling involved wrapping the infant round and round with long strips of cloth in a manner such that the arms were held extended along the sides and the legs were bound together. Furthermore, at times, the head was fastened to some sort of a stay in order to prevent it from wobbling. The general idea underlying swaddling practices was that of straightening the infant and of protecting him from injury. While the general nature of swaddling in Europe is clear from works of art and from casual references to it, I have nowhere been able to find a very adequate description of how it was actually done, or of how tightly the infant was bound. Salzman (19) states that the infant was able to move only its head; Crump (5) gives some description of the practice as does Buffon (2). The condition of the swaddled infant was such that, according to Watson's theory, the infant continually should have expressed rage. It has not been possible to find a single statement concerning the newborn infant's reaction to the swaddling process. Tiedeman, it is true, mentions a slight repugnance to swaddling on the part of his son, but this was when his son was several months of age. If children had uniformly cried during most of the time that they were swaddled that fact could hardly have escaped observation, nor, I think, could the practice have endured for centuries.

But we are not dependent upon European sources for information concerning reactions to swaddling. The binding of infants occurs in many parts of the world other than Europe. It was practically universal on the continent of North America at the time of its settlement by white men. The early explorers frequently mentioned the binding of the American Indian infant, probably because it differed in one or more respects from European binding. One of these differences consisted in the fact that whereas the European child was swaddled and then lain in a crib or on a bed, the swaddling of the Indian infant bound him to a rigid or semi-rigid cradleboard. The Indian infant was more restrained than was the European child. The accounts of various travelers, explorers, missionaries do not indicate that the Indian infants objected to this treatment. In fact, many observers commented on the good behavior of infants when thus bound.

In some sections of America, many aboriginal customs still persist, and one can observe at the present time the reaction of the Indian infant to his bindings. I have been interested in making such observations among the Hopi and Navaho Indians of Arizona and New Mexico. Since these observations have been published in detail elsewhere (10, 12) they need be mentioned only briefly here. They show beyond doubt that the young Indian infant does not cry when bound, although his limb movements and bodily movements are very greatly restricted.

In the Southwestern culture area, the head of the infant remains free to move. But among some Indian groups, in the past, even the head of the infant was fixed. This condition was imposed as a part of the process of head-molding or head flattening, which was most prominent along the Northwest Coast, particularly among the Chinook. The procedure made the head completly immobile. At the same time, the limbs were encased by bindings. The flattening of the forehead, so as to make the head more or less wedge-shaped, was achieved by placing on the forehead a hard pad, which was pressed against the forehead by means of the thongs which passed through the floor of the cradle and were tied. The thongs were kept tightened to what was considered to be just the right degree, the adjustment of them often being entrusted to skilled and experienced individuals. The resulting head shape was a mark of high social station and was considered to lend beauty. The process began shortly after birth and was continued, almost without interruption, for several months, often from eight to ten months. The head-molding technique has been described for various Northwest Coast groups by Kane (16), Swan (22), Cox (4), and Gunther (14). It is not followed at the present time.

Most of the writers cited above have said nothing about the behavior of the infant who is being subjected to this treatment. This, in itself, would suggest that the infant's behavior was not unusual. Bancroft (1) and Cox (4), however, stated that the infants seemed to feel no pain, and Kane (16) observed, "I have never heard the infants crying or moaning. . . ." Kane's interpretation, however, was that the infants may have been partially stupefied by the procedure. The eyes are described as bulging as a result of the intracranial pressure. The fact may be, however, that this pressure, although great, caused no pain. One can readily demonstrate to himself that pressure applied to the *forehead* does not hurt, where as pressure applied to the sides of the head, where it was administered by Watson and others, is quite painful. The temporal regions of the head are particularly sensitive to compression.

The argument up to this point may be summarized as follows: The newborn and slightly older infant has no unique pattern of reaction to restraint. He reacts to all strong and persistent stimuli with restlessness and crying, a crude pattern of activity which is similar for all sorts of intense and enduring stimuli. The effective aspect of the stimulations employed by Watson, Sherman, Taylor and ourselves was probably strong pressure or pain. On the other hand, methods which achieve restraint of movement without steep gradients of pressure—such as the conditions which are found in utero, in swaddling, and in binding to a cradleboard—do not produce restlessness and crying. These statements, I believe, will harmonize all the seemingly contradictory evidence which has been presented.

Let us turn now to a consideration of the older infant. In Watson's theory, the negative reaction to restraint is characteristic not merely of the neonatal period but it is a permanent part of the human makeup. In *Psychological Care of Infant and Child* he stated that no amount of training will ever completely eliminate the response.

THWARTING

In later infancy, everyone knows that "thwarting" of various sorts will lead to something which may be called rage. The early baby biographies are full of instances of reaction to frustration. For example, it was recorded that one baby cried because he was not permitted to pull his grandfather's beard. Such stimulation is "restraint" only in a metaphorical sense. It is likely that Watson knew that such responses occurred, and his research may be interpreted as an attempt to find an instinctive basis for the responses to frustration. We have seen that he was probably wrong in tracing them back to the neonatal level. Rage, I believe, is a reaction which is not present until purposeful behavior has been developed and is foiled in attaining its end. It is unlearned in this sense, that when the child has developed a purposive sequence of behavior which can be interfered with, he will exhibit "rage" on the first occasion on which this interference occurs.

It was possible to observe early reactions to thwarting in the case of the two infant subjects previously referred to, since they lived under controlled conditions which made it possible for us to know when a particular activity first suffered interference. As soon as the infants became accustomed to playing with a rattle, which was not offered them until the eleventh month, each twin cried when her rattle was taken away. Another type of thwarting arose as follows: When we picked up the infants, it was usually for the purpose of feeding them or bathing them or experimenting with them. But occasionally we picked up an infant who was quiet and contented and returned her to the crib without further ministrations. In these circumstances she invariably cried. Furthermore, if we failed to experiment with either child at the usual time of day, she became restless. This may also be looked upon as a form of frustration, or at least as an interference with the usual routine.

Of course, physical restraint of movement may be employed as *one* means of frustrating the older infant. When our twin subjects were several months of age we restrained their arms by removing the arms from the sleeves of their garments, and buttoning the clothing over the arms so that they could not be employed as usual. This did not exert pressure but it made it impossible for the subjects to carry out their ordinary manual activities. This situation, of course, led to restlessness and crying.

Some Pueblo Indian infants at several months of age (10) begin to object to being placed on the cradleboard. But it is important to note that the Indian infant of this age has been off the cradleboard each day for an interval sufficient to accustom him to manual play and to other activities. The negative reaction to the cradleboard which sometimes occurs in older infants may be due not merely to restraint but to the prevention of customary activities.

Physical restraint of movement does not *always* cause negative reactions in the older infant. In the case of the two infant subjects to which I have already referred several times, attempts were made to test reaction to restraint in the tenth month by repeating the head and nose stimulations which had been employed in the third month.

No such stimulations had been presented during the interval. But, while Mrs. Dennis and I had not restrained the infants during this period, we had stimulated them in innumerable other ways. Not only had we fed, bathed, dried, and clothed the subjects but we had plied them with odors, tastes, sights, and sounds, as parts of experimental tests. To most of these they had reacted favorably. In consequence, when one of us leaned over the crib and held the head of either subject very firmly between our hands, her reaction was to quietly look up and *smile*. For several minutes no negative reaction occurred. Eventually the child became tired of the stimulation, but no sooner than did we. It was sometimes necessary to hold a subject's head immobile for a full five minutes before she became restless and began to fret. I feel certain that it was discomfort rather than restraint which occasioned the final fretting.

It must be emphasized that the head and nose stimulations, in this situation, were not frustrating or thwarting in character. The infants were lying on their backs in their cribs, without toys, unoccupied. Their behavior indicated that they definitely wanted us to come near them. Any attention on our part, so long as it was not painful, they welcomed. Under the circumstances, their positive reactions to the head and nose stimulations were a form of positive social reaction. In other circumstances, holding the head or pressing on the nose might have been a pronounced form of thwarting, and might have led to different results.

INTERPRETATION

Such considerations show the probable futility of attempts to test for "instincts" or native emotional stimuli after the very early months of life. Every stimulus which is presented, even if it has never before been employed, bears some relationship to the experience of the child. This is a fact which has general importance beyond the bounds of the mere topic of reaction to restraint. The child's reaction tendencies with respect to any test situation, like his reaction to restraint, can be stated, not absolutely, but only in terms of the relationship which the test situation bears to past situations and to the behavior repertoire. The child of several months of age does not react to a piece of fur, a snake, a loud noise, or a pinching of the nose as a unique stimulus, as a thing-in-itself, but he seems to place each situation into some category of stimulation. A stimulation is a playful approach, a firm oral administration, a strange object, or a frustrating circumstance. The same absolute stimulating situation may be any one of these things depending upon the history of the child. Reaction tendencies beyond the first few months cannot be said to have absolute stimuli, such as loud noises or restraint of movement but instead they have relative stimuli, such as strange objects and frustrating situations (11). What is strange and what is frustrating necessarily depends upon the stimulation history of the child.

It has been seen that reaction to frustration involves the prior acquistion of the responses which is to be thwarted. Therefore, this reaction does not correspond to Watson's notion of an instinct; that is, a native response pattern elicited by certain native stimuli. The stimuli which will elicit rage are not determined solely by the structure of the individual but in part by the child's experience. In other words, a certain kind of learning is involved in reaction to frustration.

I would caution, however, that this does not mean that this reaction is learned in the same sense that language is learned. In the past the gap between instinct and learning has been made too large. It has been customary simply to classify all behavioral phenomena with reference to this dichotomy, without attempting an analysis of the intermediate modes of development.

I believe a useful subclassification of the learned aspects of behavior can be attained by distinguishing between those things which the individual learns by himself without the aid or direction of others, and those things, like language, which are socially transmitted. The former I have called autogenous, the latter, sociogenous behavior (11). It must be said that this division still is a crude one, but it is adequate for our present purposes.

Often we have been led by our terminology to place whatever is not totally instinctive into the class of socially transmitted phenomena. As we have accumulated evidence which indicates that stereotyped response patterns with predetermined stimuli are fewer than we had supposed, we have put a great deal of emphasis upon the *social* determination of behavior. Today, no one will deny that a very large part of the behavior of the human *adult* is socially determined. But, in my opinion, we should not conclude that behavior during the first year of life also is socially determined. Most of the infant's repertoire, like his reaction to frustration, is attained through maturation plus the infant's individual experience, and does not require the intervention of other persons (11).

For it is true that the reaction to thwarting may come about autogenously. Our two infant subjects were under observation from the age of five weeks onward. In no sense can it be said that we trained them to cry when thwarted, or that we provided an example. Yet this response appeared without trial and error on the initial occurrence of each of several situations. In other words it was not a sociogenous response. To the contrary, the experiment proved it to be autogenous.

SUMMARY

In closing, I may say that I have attempted to demonstrate the following points:

1. At birth, the infant reacts with crying and restlessness to any form of intense and enduring stimulation, of which rough restraint may be one form.
2. Restraint of movement achieved without the use of intense stimulation does not cause negative reactions in the newborn.
3. At a later age, the infant will react also with crying and restlessness when some customary sequence of events meets interference.
4. Whether or not restraint of movement will cause negative reactions in the older infant depends upon whether or not the restraint interferes with customary sequences which have been built up. What is thwarting to the infant is not predetermined but depends upon postnatal events.
5. Interference with behavior sequences gives rise to negative reactions in the absence of any social transmission of this phenomenon.

Finally, I have suggested a terminology, by which some of these views may be expressed as follows: Reaction to frustration is not congenital or instinctive but it is autogenous.

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Four—The Expression of the Emotions in Infancy

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Understanding human behavior is not a simple matter. Sherman's study seems to show that in the newborn, facial expressions are only slightly differentiated if at all. Among adults, there is good evidence that most of our facial expressions are means of communication that have been learned as are our gestures, our exclamations, and our oaths. We might conclude, therefore, that there are no natural ways of expressing emotions.

But Goodenough's study, presented here, tends to show that in reaching this conclusion, we are overlooking an intermediate age period in which the child has developed facial expressions that do not occur in the newborn infant, but has not yet learned to assume the sophisticated expressions expected of the adults in his social group.

The question of the relative influence of "native" versus "learned" elements in the overt expression of the emotions has been a point of controversy for several years. For a critical review and bibliography of the material on this topic the reader is referred to Landis (2). Repeated experiments have shown that facial expression alone as shown in the ordinary still photographs of adults posed to simulate various types of emotional expression is an inadequate basis for identifying emotion, since there will usually be much disagreement among judges as to the specific emotions which the different photographs were designed to portray. Even less agreement has been found when the material consists of photographs taken in the laboratory under conditions of genuine emotional excitement or strain. These results have led some persons to the conclusion that the facial expression of emotion is in the main a learned reaction, functioning primarily as a form of language. Like other languages it may be expected to differ somewhat from one social group to another, and to become modified in various ways with the passage of time.

That the overt expression of emotion among adults may be inhibited or simulated in order to conform to social standards can hardly be doubted. It seems improbable, however, that such modification of expression either for the purpose of concealment of emotion or of conveying the idea of an emotion not actually felt will occur during the first year of life; while anyone who has ever attempted to induce a young infant to imitate an action performed in his presence may well doubt whether any large proportion of the changes in facial expression shown at those early ages have been brought about through either conscious or unconscious imitation. It seems worth while, therefore, to ascertain the extent to which adults who have had some experience with young children are able to interpret the expressive behavior of infants with sufficient accuracy to enable them to match photographs taken under conditions assumed to be provocative of some degree of emotional response with the written descriptions of the situations under which each was taken. A very careful investigation of this nature is reported by Mandel Sherman (3). Sherman's subjects, however, were newborn infants whose movements are but slightly coordinated and who, presumably, had not yet fully recovered from the shock of birth. He found that without a knowledge of the stimulus, agreement between observers as to the character of the emotion which the infant was presumably experiencing was no greater than would be expected by chance. We are not warranted in assuming that similar results would be obtained for older infants. As material for this study, a series of eight pictures selected from *Die Entwicklung der Gemutsbewegungen im ersten Lebensjahre* by Martin Buchner (1) was used. They were taken under conditions thought favorable for the eliciting of mild emotional states at an instant when the behavior of the infant suggested to the observer that the anticipated emotion had been aroused. The youth of the subject (ten months) renders it highly improbable that the character of the expression had been affected to any marked degree by social tradition. For the same reason, "camera consciousness" may safely be ruled out.

Descriptions of the situations under which the photographs were taken are given by Buchner in the monograph cited, together with a brief characterization of the emotion which the child was believed to be experiencing. These descriptions were translated literally except for the omission of certain specific references which might serve as a guide to the matching. Descriptions of four other photographs not used in the experiment were added to the list in order that the matching of the last picture would not be determined automatically by a process of elimination. A group of 68 university students enrolled in a course in child training served as judges. Each student was given a set of photographs and a mimeographed sheet of instructions as follows:

Judging Emotions from Photographs

Instructions: You will be given a series of photographs, labeled on the back A, B, C, D, E, F, G, H, respectively. Below is a series of paragraphs describing the situations under which the photographs were taken. In some instances the emotion which the child was believed to be experiencing is also indicated. Descriptions of four additional situations have been added, in order that the matching of the last picture will not be determined automatically by a process of elimination.

Look at the first picture (labeled A). Note the child's facial expression and bodily posture carefully. Then read through the descriptions of all situations in which the child was photographed. One of these corresponds to Picture A. When you have decided which one of the situations described would be most likely to elicit the expression shown in the picture, write the letter A in the parentheses preceding this description on the mimeographed sheet. Do the same for Picture B, then for Picture C, and so on until all the pictures have been matched. Four unmatched situations will of course be left over.

Do not omit any of the pictures. Make a judgment each time, even if you feel very uncertain as to the correct choice. State only one judgment for each picture.

Situation

( ) 1. Satisfied smiling (with affection). He was looking at his mother who talked to him in a friendly manner.

( ) 2. Astonishment (with slight displeasure). I counted loudly and emphatically as I walked toward him—"twenty, twenty-one, twenty-two," and so on. He looked me uncomprehendingly in the face.

( ) 3. Fear (perhaps with dislike). A strange woman struck two blocks together, growled, and rushed suddenly up to him, frowning darkly.

( ) 4. Dissatisfaction (with slight obstinacy). I had taken him up in my arms and then put him back in the chair. He wanted to come to my arms again.

( ) 5. Astonishment (with auditory attention). He is listening to the ticking of a watch.

( ) 6. Slight obstinacy. His mother wanted him to give her his hands, but he would not because she had just taken a toy away from him.

( ) 7. Crying. He was tired of sitting and wanted to come out of his high chair into my arms.
( ) 8. Astonishment (with ocular attention). A bright-colored new toy clown was shown to him.
( ) 9. Grimacing. The rogue is trying to wink.
( ) 10. Pleasure. I rolled a shining tin can on the tray. He said softly "dai."
( ) 11. Roguish smiling (with affection and tense expectation). His mother was teasing him. "Only wait, now, I'm going to catch you!"
( ) 12. Anger (and displeasure). A toy has been taken away from him.

Table I shows the distribution of judgments for each of the eight pictures. Correct judgments are shown in the diagonal line of boxes. Of the 544 judgments 258 or 47.4 per cent are correct. The chance expectancy is 8.3 per cent correct judgments. The percentages for the different pictures vary from 14.7 to 77.9 per cent. Inspection of the incorrect judgments will show that they are far from being distributed at random. In each case they tend to be concentrated within one or more situations which are as a rule closely similar to the correct one. For example, with four exceptions the errors in interpreting Picture A consist in choosing either Situation 2 or Situation 5. In both of these situations as well as in the true one *astonishment* was thought to be the predominant emotion. Likewise in the interpretation of Picture B the majority of the errors consist in selecting Situation 6 rather than 4. Again the resemblance of the two situations is striking. The only important exception to the general rule that confusions are for the most part confined to the situations in which the emotions imputed to the infant are the same or closely similar is found in the case of Picture F, which is

TABLE I

DISTRIBUTION OF JUDGMENTS IN MATCHING PHOTOGRAPHS AND SITUATIONS

Picture	*Situation*											
	8	4	9	1	11	3	10	7	2	5	6	12
A	10				1	1			33	21	2	
B		14				11			9	1	24	9
C	1	2	53	1	8	1		1		1		
D	1			43	16		6			2		
E	9		3	7	26		7		1	6		
F		11	1			14		1	1	5	30	5
G	12			3	1		50			1	1	9
H	1	5			2		3	48				

most frequently attributed to Situation 6. Here the source of the confusion is probably to be found in the child's attitude of active withdrawal. Had the judgments been based upon actual observation of the child's behavior or upon moving pictures it is probable that the comparative speed of reaction (startle reflex) would have served to differentiate the two.

SUMMARY

1. An experiment was designed to ascertain the degree of accuracy with which the expressive behavior of an infant as shown in ordinary still photographs can be identified with the descriptions of the circumstances which called it forth. A group of 68 university students enrolled in a course in child training acted as judges.

2. It was found that on the average photographs and situations were matched correctly in 47.4 per cent of the judgments, which is 5.7 times the percentage of successes to be expected by chance. Errors in matching were in the main confined to confusions between situations in which the emotions imputed to the child by the original investigator were the same or closely similar.

3. Since the subject of the photographs was an infant of ten months, it seems improbable that the form of behavior displayed by him had been appreciably modified by social tradition or by "camera consciousness." Moreover, since the still photographs show only a single brief phase of the total pattern of expressive behavior and afford no evidence as to the speed of reaction or of its vocal accompaniments, and since the stimuli used were for the most part of a comparatively mild nature not calculated to call forth pronounced reactions, the degree of success attained by the judges takes on added significance. The findings suggest that however greatly the overt expression of emotional states may be inhibited, modified, or intentionally assumed in the social relationships of adult life, the language of expression is nevertheless built upon a core of native reaction-patterns which appear at so early an age that they can hardly be ascribed to training. It seems probable that the role of training and experience in the expression of the emotions consists chiefly in a process of inhibition and substitution whereby the innate reaction-patterns take on a more or less distorted form. If this hypothesis be correct, the optimum period for the study of emotional expression is to be found in early childhood, before the original patterns of response have become too extensively overlaid by the habits resulting from social experience.

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Five—Anger in Young Children

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This is a part of a naturalistic study of the home situations in which children become angry and exhibit fits of temper, of how anger is handled by parents, and of the frequency and duration of temper outbursts. As in the case of any extensive study, a full account cannot be included here. For further details the student must turn to Goodenough's monograph.

An investigation was undertaken at the University of Minnesota Institute of Child Welfare that was designed to throw some light on the frequency, duration, causes, and methods of handling anger outbursts among children in the home. An ideal way of securing data of this kind would have been to place in each home a trained observer whose time could be solely devoted to making the observations. The cost of this method, if applied to more than one or two children for any length of time, would have been prohibitive, however. It was therefore decided to enlist the cooperation of a group of intelligent parents in making the observations.

From the outset it was recognized that it would be impossible to secure information regarding the behavior of unselected children in this way, since the making of the records would necessarily require a greater degree of intelligence and education than is possessed by the average parent in the general population. The inquiry was therefore restricted to children whose mothers had had some college training or its equivalent. More than half of the mothers taking part in the study are college graduates. Whatever conclusions are drawn from the findings must therefore be restricted to children reared in homes of superior culture by parents whose interest in child development training is sufficiently great to render them willing to devote time and thought to the carrying out of an investigation of this kind. Although any comparison with unselected children must of necessity be speculative, an *a priori* judgment would lead to the expectation of a somewhat lower incidence of outbursts among children from such homes than would be found for the general population.

PROCEDURE

Through the coöperation of the Parent Education Department of the Institute of Child Welfare the project was described to the mothers enrolled in the study groups of the College Women's clubs of Minneapolis and St. Paul. Sample record blanks were distributed in order to give a concrete idea of the amount of time required for keeping the records. After a brief discussion of the purpose and value of the study the mothers were asked to volunteer to keep records of the anger outbursts shown by their children. The original request as made to these groups called for records over a four-month period, since it was thought that it would be interesting and valuable to ascertain whether the keeping of such records would affect the methods used by the mothers for controlling anger and perhaps modify the frequency of the outbursts in the course of the period of recording. As will be shown later, no such effect could be observed. In later requests made to other groups the period for which records were to be kept was therefore reduced to one month. It was found that a fairly long record was needed in order to secure sufficiently reliable data for each child to warrant the making of comparisons between children.

Each recorder was provided with a set of typed instructions for keeping the records. The instructions and the record form had previously been tried out by a small number of volunteers and by observations made in the Institute nursery school.

CONDITIONS MAKING FOR INCREASED IRASCIBILITY

In this section we are not concerned with the immediate or direct causes of specific outbursts of anger but only with the more general conditions that seem to affect irascibility. Possible factors of this kind are the amount of sleep taken, the child's general physical condition, special or unusual occurrences in the home, the size and composition of the family group, the birth order of the child, the hour of the day, the day of the week, and the like. It should be noted that the establishment of a relationship between any or all of these factors and the frequency or duration of anger outbursts does not always afford evidence as to the essential nature of the relationship. An increased number of outbursts may be due either to the increased irritability of the child himself, causing him to become disturbed over minor conditions that would ordinarily be ignored, or to external factors involving an increase in the irritating conditions to which the child is subjected. Variations in the frequency of anger may thus be brought about either by internal conditions resulting in differential responses to constant stimuli or by external conditions affecting the frequency and intensity of the annoyances to which the subject is exposed. Although the nature of the factor under consideration in many instances enables us to infer with a fair degree of certainty whether its major influence is external or internal, there are other cases in which either or both types of influence may be at work.

In considering the relationship of temporary conditions of poor health to the frequency of outbursts, only those children for whom the condition under consideration was reported at least once during the period of observation have been included. Thus there were eight children for whom only normal health conditions were reported over the entire period of observation. These have not been included in any of the following comparisons. There were 35 children for whom a slight cold was reported at least once during the period of observation. For these 35 children the mean number of anger outbursts on 264 days when a slight cold was said to be present is 1.20. The mean number of anger outbursts for the same 35 children on 1,375 days when health is reported as normal is .77. This difference, which is 6.70 times its standard error can hardly be attributed to chance.

Digestive disturbances, chiefly of a minor nature, are reported for 16 children on 40 days. The mean number of outbursts per day under these conditions is .90, while for the same 16 children on 694 days when health is reported as normal the mean number of outbursts is .81. While a slightly greater number of outbursts are reported, on the average, when digestive disturbances are said to be present, the difference is only .49 times its standard error.

Other conditions of poor health or of disturbed physical condition include a number of minor accidents, such as unusually severe bumps and falls, a dislocated shoulder, a needle broken off in the child's hand, and a small number of infectious diseases such as chicken pox and whooping cough. The total number of days included in this rather miscellaneous group of adverse physical conditions is 47; the number of children represented is 14; and the mean number of outbursts per day is 1.61 as compared with a mean of .70 for the same children for 922 days when health is reported as normal. The difference between the means is 3.27 times its standard error, a difference that would occur by chance less than once in a thousand times.

The number and character of bowel movements has been reported very regularly for all cases. On only 69 days out of the total 2,124 is the item left blank, and since the distribution of the outbursts for these days follows the normal pattern, it is probable that the occasional omissions may be attributed to chance. We have grouped the records into the following general classes:

1. Constipated. This includes days on which it is definitely stated that no bowel movement occurred, unless two or more were recorded for the previous day. The second and thereafter of two or more consecutive days are also included when not more than one bowel movement occurred if the amount is said to be scanty and the consistency hard.
2. Diarrhetic. We have considered the case to be one of diarrhea if three or more bowel movements occurred on a single day without the use of a laxative and the consistency was said to be soft or watery. We have also included cases in which only two bowel movements occurred if the amount was said to be unusually profuse and the consistency very soft and watery.
3. Normal. All cases other than those described above have been classified as normal.

By using the above classification it was found that the mean number of outbursts per day for 119 days when constipation was indicated was 1.24. This is based upon the records of 22 children, the others showing no constipation. The mean number of outbursts for the same children on 1,039 days when the bowel condition was normal was .81. The difference between the means is 4.38 times its standard error. We are thus warranted in assuming that children tend on the average to be distinctly more irritable when they are constipated than when the bowels are in a normal condition. Diarrhetic conditions were reported for only 10 children for a total of 24 days. The mean number of outbursts per day is .92 as compared with an average of .86 for 576 days when the bowels are normal. While the average number of outbursts during diarrhea is slightly in excess of the number occurring when the bowels are considered normal, the difference, which is only .26 times its standard error, is well within the limits of chance. It should be noted, however, that the number of cases is small and in no case is there any indication that the condition was other than a very mild one.

The hour of the day at which the outbursts occurred is stated in 1,842 of the 1,878 outbursts reported. Table I and Figure 1 present the relationship of the frequency of outbursts to the hour of the day at which they occurred. In the second column of Table I is shown the total number of times the hour in question was included in the records, that is, the total number of times a child was awake and under observation at the hour specified. The third column shows

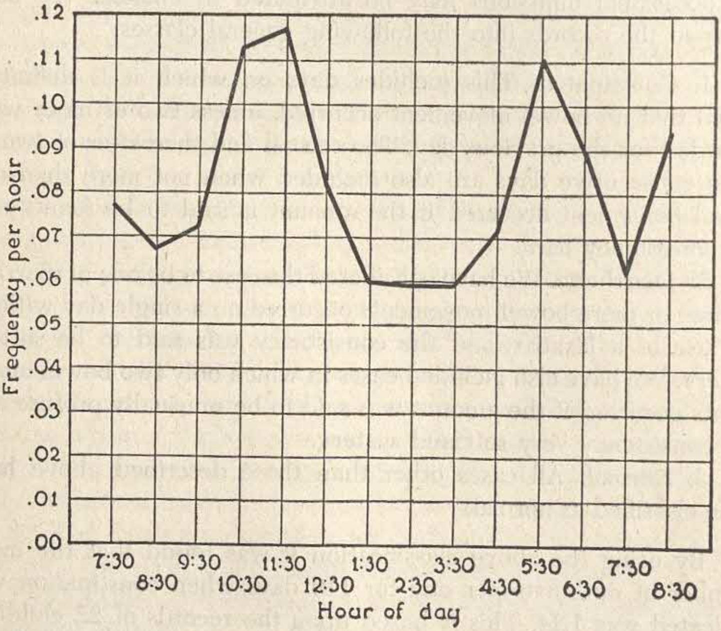

Figure 1. Relationship of frequency of outbursts to hour of day.

the total number of outbursts occurring at each hour. The fourth column shows the mean number of outbursts per hour, reckoned by dividing the number of outbursts at each hour by the number of times the hour was included. It will be observed that outbursts are relatively as well as absolutely much more frequent at certain hours than at others and that the manner in which the outbursts are distributed over the day appears to bear a distinct relationship to the usual meal hours.

It is much to be regretted that the daily record form did not call for a precise statement as to the time of the meal hours. Failure to secure this material makes it impossible to relate the changes in frequency to the meal hour with complete assurance, but the probability that this is the determining factor is very great since there is no other obvious reason why the distribution should take the form shown. An examination of the hours reported for outbursts occurring at mealtime affords a rough indication of the usual hour when meals occurred. The breakfast hour seems to have varied considerably from child to child. Some of the children were in the habit of rising at six or even earlier, others not until nine. As a rule, breakfast came fairly soon after the child was dressed.

The internal evidence of the blanks leads us to the opinion that if it had been possible to relate the hour of the outbursts more definitely to the meal hour, a much greater proportion of the early morning episodes would have been found to occur before rather than after breakfast. The noon meals of most of the children seem to have taken place at twelve o'clock or somewhat earlier, and the most frequent time for the evening meal is about six o'clock. A number of the children were given their meals in advance of the other members of the family. This is particularly true of the younger children.

TABLE I

Relationship of Frequency of Outbursts to Hour of Day

Hour	*Number of Times Hour was Included in Records*	*Number of Outbursts*	*Mean per Hour*
Before 8:00 A.M.	2,036	156	.077
8:00- 8:59	2,082	139	.067
9:00- 9:59	1,821	132	.072
10:00-10:59	1,594	180	.113
11:00-11:59	1,619	189	.117
12:00-12:59 P.M.	1,855	153	.082
1:00- 1:59	1,421	85	.060
2:00- 2:59	1,148	68	.059
3:00- 3:59	1,522	91	.059
4:00- 4:59	1,713	121	.071
5:00- 5:59	1,756	194	.110
6:00- 6:59	2,018	182	.090
7:00- 7:59	1,513	94	.062
8:00 and after	618	58	.094

An examination of Table I shows an initial peak in the frequency of outbursts before eight in the morning, followed by a drop between eight and nine. Thereafter the outbursts steadily increase in frequency until about noon, after which there is an abrupt decrease both in absolute and relative frequency. They remain at a low point until around four. Thereafter an increase in frequency appears and continues up to six o'clock after which there is a second drop, which continues until about eight. After eight a rapid increase in frequency of outbursts takes place in the children who are not put to bed before that time. The popular idea that a full stomach makes for contentment and placidity seems to be experimentally verified by our data. It is not improbable that a light lunch in the middle of the morning and again in the middle or latter part of the afternoon would be of material assistance in reducing irritability, at least in certain cases. It may be noted here that, judging from the internal evidence of the records, the greater number of parents in this group appear to have been rather meticulous in enforcing the rule against eating between meals.

We thought it possible that outbursts might tend to be more frequent on certain days of the week than on others, owing to differences in the family schedules from day to day. No such relationship, however, was apparent. The greatest number of outbursts occurred on Tuesday, the fewest on Thursday, but the differences were only such as might be expected by chance.

Six—Children's Fears

ARTHUR T. JERSILD

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Jersild and his collaborators have conducted a very extensive investigation of children's fears, with special reference to children of school age. The full reports of their studies are cited in the references at the end of this section. The following is a nontechnical summary.

In these works (1, 2, 3, 4) a number of different methods were used, some of which represent a departure from conventional methods of research. Mothers of 150 young children and a number of teachers and nurses cooperated by keeping records of children's fears at home and in other situations; 400 children of elementary school age were questioned with respect to their fears; another group of 300 adults supplied written anonymous reports of fears they recalled from their own lives during childhood. Other projects included follow-up studies of some children, controlled experiments, and studies of methods of overcoming fear.

CAUSES OF FEAR

A matter of particular interest in this study is concerned with the things that children seem most often to fear at various age levels. Among the stimuli which were found most frequently to arouse fear in the child during the first year of life are the following: noises, strange events, sudden, rapid, or unexpected movement, painful shocks, and events associated with pain and falling or danger of falling. These categories represent about 90 per cent of all "fears" observed during the first year.

These findings regarding early fears raise the questions as to what are the "original" stimuli that elicit fear. Once the view was widely held that there were two and only two "natural" and unlearned fear stimuli, namely, noise, and displacement or loss of support. This theory has been found to be inadequate. First, it is based on the assumption only that is natural which happens to be apparent at birth or soon thereafter, and thus does not adequately take into account the changes in behavior that occur not through learning alone but through the joint influence of maturation and learning. Second, the theory takes too narrow a view of what is an effective stimulus. In the case of noise, for example, it is necessary to consider not only the noise but the total situation in which it occurs, for similar sound waves may cause fright under some conditions and not under others. Third, empirical study shows that the stimuli that elicit fear early in life are not limited to noise or displacement alone but include any sudden, intense, unexpected, or novel stimulus, from any source, impinging upon the organism through any or all of the sense organs. Even this general statement, however, leaves many questions unanswered. To analyze the susceptibilities of individual children and to define the peculiar combinations of stimuli that elicit fear requires more extensive research than is yet available.

As already noted, the fears of the infant revolve around tangible events in the immediate environment. With increasing age this condition is decidedly changed. As the child grows older, month by month, his powers of discrimination and perception expand. Things take on meanings they did not have before. Around the age of two, perhaps earlier or a little later in some cases, his world of fancy begins to take form. He becomes capable of fantasy by day and dreams by night, and as his imagination comes into play, the range of his fears grows larger and larger.

This trend is accentuated from the age of two to the age of five. More and more the child becomes impervious to noises, novelties, sudden happenings, or other tangible events. There is an increase in fears of accidents that might occur at a future time even though there is no immediate danger or threat of harm. There also is an increase in fears of the dark, of being alone, of imaginary creatures, of wolves, of kidnappers, and of matters associated with death. By the age of five and increasingly thereafter many children exhibit fears under the general heading of fear of failure, of ridicule, of loss of prestige, and of events that might threaten their status and their future security.

Among the miscellaneous reactions during late preschool years are fears akin to stage fright. Stage fright is incidentally an interesting phenomenon. It is one of a family of fears, indications of which can be found as early as the sixth month of life when some babies shrink from the approach of a stranger. It almost seems, at times, that the child is reacting to the stranger as though he were an enemy, and we find similar fears on a more sophisticated scale as the child grows older. It is rather ironic, this fear. We might ask whether its roots had been planted in a remote era when savage ancestors would attack and eat the young of their own species. Or is this fear of others something which our culture has imposed upon the child? There is no adequate answer to these questions. There is, however, some reason to believe that aspects of fear are inborn, although it is also true that it is fostered and aggravated by competitive pressures. Much of this fear could, no doubt, be forestalled by proper guidance in early years.

If we study the child in a still later phase, we see a continuation of much the same picture as was found at five. A child of elementary school age entertains many fears of immediate and likely dangers, but these constitute only a fraction of the dangers which enter his fancies. When several hundred elementary school children in one of the studies now under review were asked to tell about the worst thing that ever happened to them, the results showed that there was only a small degree of overlapping between actual misfortunes and the fears they had described. "Worst happenings" included the hurts and pains and disappointments of everyday life. But a majority of the fears reported went far beyond this and dealt with imaginary or remote dangers, such as wild animals, bogeys, kidnappers, and the possibility for example, that *if* one traveled by ship, the ship *might* sink.

This trend in the children's fears was apparent again in the data obtained from adults who described as best they could the fears they recalled from childhood.

These developmental trends in children's fears are prominent, but this does not mean, of course, that every child conforms to them. There are many individual differences. Even when this is recognized, however, there still is impressive evidence of the reign of fear. It is somewhat anomalous that immunity to fear does not increase apace with increasing growth. The fears that assail the infant in the cradle are limited in scope compared with those that come with time in the case of many children. As the child grows able to cope with things within the horizon he becomes more and more susceptible to fear of the things beyond his horizon. Savages and beasts that endangered his forefathers long ago come out of their lairs and caves again to pursue him. If he escapes such fears, he may be troubled by other dangers that never are likely to occur.

Such information as we have indicates that many fears of such fictitious dangers are carried into adult years. Our fears as children and as adults are much like the experiences about which Mark Twain spoke when he said that he had had many troubles in his life but most of them had never happened. Undoubtedly, many such fears, although seemingly irrational, serve merely to focalize, or even to disguise, an underlying condition of emotional insecurity or conflict.

EFFECTS OF FEAR

The foregoing suggests two questions. First, what is the value or utility of this load of fear? Second, assuming that it should be done, what are methods of preventing or overcoming fears?

The problem as to the utility of fear raises many issues that cannot be considered at this time. That even a seemingly irrational fear may have beneficial effects is true. But it is also true that many of these benefits might better be effected by less painful forms of motivation.

Apart from this, a serious question might be raised as to the value of the effects of fear even when there seems to be a good reason to be afraid. As is well known, intense emotional excitement is accompanied by profound bodily changes. Such changes are regarded as part of the mobilization of energies for an emergency, to help the individual to fight or to flee in the struggle for existence. Most persons have at one time or another experienced some of these symptoms, such as the racing pulse, dry mouth, digestive retardation, which the psysiologist Cannon has described. As civilized human beings we have machinery to increase physical strength, speed, and endurance—to help in the primitive struggle for existence against tangible foes. But, actually, the emergencies we face when we are afraid in modern life more often call for quick wits and good thinking than for strong arms and fleet legs. The person who is afraid to take an examination may be so well charged with energy that he could outpunch and outrun the fleetest and strongest instructor. But prowess of this kind does not help to pass an examination.

In order to test the effect of these physiological changes on mental efficiency, I performed an investigation in collaboration with Dr. W. S. Thomas of St. Luke's Hospital. We tried to induce physiological changes resembling to some degree those accompanying fright by means of artificial injections of adrenalin. We had only six subjects in this study, and the experiment leaves much to be desired on other points as well. When the effects of adrenalin appeared, such as pallor, tremor, and changes in pulse rate and blood pressure, the subjects were put to work on mental and motor tasks. We found that they were somewhat more competent than usual in motor tasks but on mental tasks they were somewhat less efficient. In the excess of physical excitement, the subjects often seemed to fumble, to stutter mentally, so to speak, and often in the increased pressure to work rapidly they made more mistakes. Moreover, the subjects were more irritable than normally and several reported that they suffered from aftereffects of fatigue and discomfort. This study was limited in scope and is not conclusive, but it does suggest that the energies mobilized during emotional excitement oftentimes hinder rather than help a person in an emergency.

OVERCOMING FEARS

The second practical question that was raised concerns the subject of overcoming children's fears. There is space here for only a few of the findings. An incidental difficulty in dealing with fear is that a child's fears are likely to pass unnoticed. If a child is angry or rebellious, he is likely to attract attention; but if he is afraid, his sufferings are often confined within himself and he is left to struggle alone.

Our findings show, first of all, that much by way of eliminating fear would be accomplished if adults would stop playing upon fears as a means of disciplining or intimidating the child. Over and over our data reveal the extent to which parents and teachers are guilty in this respect. Our results also include many records of specific methods frequently used in helping children to overcome their fears. The most frequent method is to try to talk a child out of his fears, but words usually avail little against an emotion. Other methods frequently reported include efforts to make a practical demonstration that there is nothing to fear, or to set an example of fearlessness, or to compel the child to face the feared event by physical compulsion or by ridicule, or to try to effect "positive reconditioning" by associating the feared stimulus with an attractive stimulus. All these methods sometimes succeed, but more often they do not.

According to the results obtained in our studies, the important rule in overcoming fear is to bring the child into active and successful encounters with the thing that he fears. One important help toward this end is to use the principle of the graded approach, to ease the child into the situation, to let him cope with it by degrees.

In the daily routine of home and school, a resourceful adult can find countless opportunities for the use of this technique.

Even more helpful is the cultivation of skills. Over and over again the findings emphasize the importance of skills, skills broadly construed to include competence in motor activities, skill in social dealings, school subjects, and countless other matters. The child who is inept merely in the sphere of motor activities not only is likely to develop fears regarding his personal safety but also, by reason of his ineptitude, to develop fears that affect his social relationships and his confidence in himself.

The principle of prompting the child to have active dealings with a feared situation was found to be helpful even in the overcoming of fears of imaginary dangers. One reason why fears of the dark and the mysterious are so persistent is that it is difficult to cope with them directly. It is difficult to get a chance to wrestle with a ghost. Yet our results indicate that it is often possible to do just that, although by indirect means. One child in our study was afraid of an imaginary dog. This creature hounded him, troubled him when he was alone, and haunted him in dark places. When his resourceful mother observed this, she decided to take the situation in hand. She first watched the child in his make-believe play. Gradually she entered into this play herself, and joined the child in games of let's pretend. Gradually, also, she began to introduce the imaginary dog into the make-believe play. In time, the child took the dog with him into closets and elsewhere, all as part of the play pattern, and eventually the dog was changed from a sinister creature into a friendly pal. Our data afford other illustrations of this principle, and many further instances in which the principle will apply can be found in everyday life. Almost any adult can recall a fur coat or a closet, or a trunk, or an attic, or a cellar, or a hayloft that was the habitat of ominous things during his childhood. Our results indicate that the mere matter of bringing the child into active and habitual and successful encounters with concrete things such as these that serve as the physical habitat of a specter may be effective in laying the specter itself.

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VIII–PERSONALITY

One—Negativism in Preschool Children

MARTHA M. REYNOLDS

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Some personality traits reveal definite trends with age. Reynolds shows that negativism in the situations she used decreases markedly between the ages of two and five.

SELECTION OF SUBJECTS

All experimentation for this study was carried on by the writer from the middle of March to the end of May, 1927, a period of about 10 weeks. The chief source of the cases was the Institute of Child Welfare Research, of Teachers College, Columbia University, whose generous cooperation enabled the writer to secure 112 children. Of these, 42 were from the Nursery School conducted by the Institute, and 70 from the Psychological Clinic of the Institute. This clinic for the last three years has been making research studies of children of preschool age, and the facilities which it afforded were of the greatest assistance.

To supplement these cases, seven day nurseries were canvassed, as well as the kindergartens of the Horace Mann School. The day nurseries furnished 120 cases and Horace Mann furnished 23.

In all, 255 children were observed. Fourteen of these were the subjects of the preliminary experimentation, and hence were excluded from the cases finally selected. There were 12 other children for whom no IQ's could be obtained. A total of 229 cases were included in the results treated statistically in this study.

DESCRIPTION OF TECHNIQUE

Materials

The only materials were a set of The Embossing Company's Color Cubes, No. 14 (36 in a box). They are one-inch cubes and are painted gay colors, each block having a white, a blue, a red, and a yellow face, as well as one face of red and white arranged diagonally and another of blue and yellow treated in the same way. The lid of the box is gay and shows designs which can be made with the blocks.

Room Arrangement

A place as secluded as possible from interruptions was selected and an effort was made to remove all distracting objects from sight. A small table, with two small chairs arranged on either side of it, was placed where it could be seen from the door and the blocks strewn out upon it in enticing fashion. The box was put as much out of sight as possible. Ideally, the table should have had a desk drawer in which the box should have been kept until it was needed.

The examiner wore a smock with ample pockets in it, so that pencils could also be kept out of sight when not in use. The stop-watch was worn on a ribbon around the neck and it was either held inconspicuously in the examiner's hand or hidden in the folds of the smock. Experience showed that such minute precautions were necessary in order that no distractions should be introduced to serve as upsetting influences.

Recording Blanks

Special recording blanks were devised. Care was taken to have them of two different colors, pink and blue, so that no mistake would be made in assigning the child to its particular group. A column was arranged on the left of the page where the time was recorded at stated intervals.

Plan of Recording

In 11 of the 13 situations, a plus sign was used to signify that the child complied with the examiner's request in the time allowed and a minus when he failed to do so. Besides this, observational notes were recorded by means of numbers to designate different types of responses and letters to signify the child's attitude. Unusual occurrences or sayings were recorded in the extra space allowed on the blanks. In the two situations where the plus and minus signs were not applicable, detailed notes of the child's reactions were taken. The bulk of the recording was done at odd moments between the situations or during them but, in any case, only a few seconds at a time were devoted to this except during the two-minute neglect period when the examiner could be legitimately employed in this fashion.

BLOCK BUILDING

Procedure

The examiner chose four blocks that stood apart from any project which the child might be working on, and putting one of them on top of another, said to the child, "Let's play blocks this way, John. See, I'll put a block on top, like this, now you put one on." She suited the action to the word, thereby making a small two-block tower, and as she finished speaking she held one of the blocks out to the child. The child was then supposed to take the block and either to place it upon the tower which the examiner had made or to begin a new one in imitation of hers. Care was taken to have the four blocks used for this sufficiently removed from the child's own project, so that the examiner could assure herself that the child was building in response to her request, and not merely continuing his own project.

After the invitation to "play blocks this way," the examiner held the block outstretched for about five seconds and then laid it down near the child in a conspicuous place. She then turned her attention to recording for the remaining five seconds. If at the end of ten seconds the child had made no move to comply, the formula and the accompanying performance were repeated.

For the third and fourth repetitions the formula was varied to: "Come on, John, let's play this way just once and then you can play your way. See, I'll put a block on top, like this, now you put one on."

When at any stage of the game the child did as requested, the examiner said, "Good! That's fine!" noted the time that the child did it and promptly passed on to the next situation.

If the child did not comply after four repetitions, any further procedure was suited to the circumstances and the child's mood, in an effort to get him to do as requested. However, experience proved that any variations should be in the form of a game, requiring differing degrees of patience and watchful waiting on the part of the examiner.

Recording

For each request made of the child, the examiner recorded a plus sign if he put the block on top and a minus if he did not comply.

If further procedure was instituted, it as well as the child's reactions was noted in detail.

SURRENDER

Procedure

This situation is more accurately described as surrender of personal liberty and consisted in the examiner's picking the child up without explanation and holding him on her lap for one minute. As soon as she was settled in *his* chair with the child on her lap, she said, "Go ahead, play with the blocks some more," and put her hands on the edge of the table to keep him from falling off and also to serve as a barrier if he should try to get down. No explanations or comments were made in answer to the child's protests. If the child exerted sufficient pressure to remove the examiner's hands from the edge of the table, he was permitted to squirm himself down from her lap. One minute was the time set for holding him on her lap, but if he got down before this was over the time was noted and the series continued as if the full time had elapsed.

Recording

Almost always the recording of this test had to take place after the child had been put down again, since the business of holding him on one's lap made it well-nigh impossible while the situation was in progress. No plus or minus scores were given here, but the time that the child played with the blocks after being settled on the examiner's lap was noted as well as the length of time that he remained there. His reactions were also recorded at several intervals throughout the situation.

IMITATION

There are three parts to this situation—waving hand, shaking head, and clapping hands. The child was requested four times to do each one. Each time a request was made, the examiner endeavored to get the child's attention and then repeated the appropriate formula. She then looked directly at him for only a few seconds and spent the rest of the ten seconds allowed for the response in writing up the record and watching him from the corner of her eye. The reason for this procedure, as explained earlier, was to relieve the embarrassment which is often shown by children of this age when they are asked to imitate. It occasionally happened that a child would not perform the desired actions while the examiner watched him but as soon as she diverted her gaze, the child would execute the movements. For this reason, standardized technique allowed for this possibility.

Procedure

The examiner said to the child, "John, do this," meanwhile performing the desired movement. For waving the hand, she waved her right hand in the air above her head; for shaking the head, she shook her head slowly from side to side; and for clapping hands, she clapped her hands together in front of her. The formula for the requests was as follows:

1. "John, do this."
2. "John, do this."
3. "Come on, John, do this."
4. "Oh, John, come on, you can do this, can't you?"

Each one was applied in order to the movement which was requested, if the child failed to comply with the previous request at the end of 10 seconds. Whenever the child made any movement which looked as if he were trying to do as requested, the examiner said, "Good, that's fine," and went on to the next part of the imitation situation or to the rapport following it.

Recording

For every request to which the child did not comply, a minus sign as given, but whenever he did what was wanted, a plus sign was recorded. The time for the three parts of the imitation situation was lumped together and merely the beginning and the end of the situation as a whole were noted.

DIGITS

Procedure

This was identical with that for the imitation situation, except that in this case the child was asked to repeat numbers rather than to imitate movements. It began by the examiner saying, "John, say 'two,'" and progressed through the four formulas until they were completed. If at any stage of the procedure the child made any attempt to say the number, the examiner immediately went on to the next number. The following numbers were used: two; three; six–four; eight–five. (In the last two cases, two numbers were presented as a single task.) As soon as the four formulas had been used for one number, the next number was introduced.

Recording

Again a plus sign was used to indicate that the child had repeated the number and a minus sign when he had failed to do so. A child who counted, even though the counting included the desired number, was scored minus, since there was evidence that he *could* say the number but that he would not comply with the examiner's request. Also, a child who said, "I can't say 'six–four,'" for instance, on the same line of reasoning was scored minus.

The quantitative element was judged by the number of refusals which a child made during a series. The refusals to the first requests were considered significant, and they were also objective. As a rule they did not require interpretation and they yielded an indication of how frequently the child reacted in a negative way to the situations presented. This method had the further advantage of being comparable for every child, that is except in the unusual cases where it was impossible to complete the series, each child was offered 13 chances to refuse or to comply. Many of the children were given several other chances. When the first request was refused, a second one was made. But, of course, for the children who complied at once, the other chances were not necessary. For these reasons it was decided to emphasize these first refusals in the plan of scoring and to give a score of 1.0 for each of them recorded for the child.

The term "first refusal" was used to designate a refusal to the first request for each of the 13 situations. Where a situation had more than one part, as in the imitation and digit situations, the 1.0 was divided among the several parts. The child's *negativistic score* was obtained by adding together the scores which he had received on each situation. This formed a measure with which available data about the child, such as IQ, CA, and parent's and teachers' judgments were correlated.

AGE DIFFERENCES

The data on the reactions of the different ages to the series of situations offer some interesting comparisons which may be expected to throw light on the question of the age at which negativism reaches its peak of development. It is one of the problems which has great practical interest. If there is a waxing and waning of negativism in the preschool years, it would be of value to know at what period of the child's development to expect it.

Do three-year-olds ordinarily exhibit the distressing habit of saying "no" to everything? Or is this the behavior which we should expect of the two- or the four-year-old? Is the "I won't" from the four-year-old a more serious deviation from the usual response than the same refusal in the two-year-old? The practical value of this information is evident. The answer to these questions should lead to more intelligent management of the different ages.

Table I shows the distribution of the children in each age group. It will be noted that a half year interval is included in each group. For instance, there were 35 children who had passed their second birthday and who had not yet reached two years six months. This grouping is used throughout, except where a few extra cases above and below the desired age range strayed into the study. Here, "below 2" means one year eleven months, and "above five" includes 12 cases from five years, no month through five years five months, and one case whose age was exactly five years six months.

TABLE I

Distribution by Age Groups of the 229 Cases in the Study

Age Group	*Number of Cases*
Below 2[a]	3
2-0	35
2-6	37
3-0	35
3-6	35
4-0	33
4-6	38
Above 5[b]	13
Total	229

[a] These cases were all at 1-11.

[b] This includes 12 cases between 5-0 and 5-5 and one of 5-6.

Throughout the discussion the age groups will be designated by the lower limit of the interval. Where the 2-0 group is mentioned, it will be understood that the group from two years through two years five months is referred to and similarly with the other age groups.

The six-month interval was used because any finer division reduced the number in each group too much and did not show up any differences in the reactions which were lost in the larger grouping. A grouping of three months was tried out for the two-year-olds and, since it yielded results on the fewer numbers comparable with the six-month group on the larger numbers, it was considered of doubtful value.

The results of the negativistic scores of the age groups may now be considered. Table II gives the mean scores on the series of negativistic situations made by each of the ages. A glance at these shows that the tendency is for the scores of the younger children to run higher and for those of the older children to be lower, although the actual ranges within which the scores fall overlap very greatly.

TABLE II

RANGE, MEAN, AND S.D. OF NEGATIVISTIC SCORES OF AGE GROUPS OF THE 229 CASES IN THE STUDY

Age Group	* *Range*	*Mean*	*S.D.*
Below 2	0	8.00	0
2-0	1 to 10	6.44	2.59
2-6	1 to 12	5.80	2.61
3-0	0 to 10	4.61	2.63
3-6	0 to 9	4.19	2.55
4-0	0 to 11	3.26	2.45
4-6	0 to 4	2.26	1.12
Above 5	0 to 6	2.42	....
All Ages	0 to 12	4.38	2.76

* All ranges are inclusive.

It will be noted that the mean negativistic score for the group as a whole is 4.38, while that for the 2-0 group is 6.44. Another interesting feature is that the mean for the 4-6 group is 2.26. In fact, there appears to be a gradual *decrease* in negativistic score from that of the 2-0 group to that of the 4-6 group.

This same result is found in the Pearson r between chronological age and negativistic score which is $-.53\pm03$. This correlation is not very high, of course, but it is high enough to be interpreted to mean that as the chronological age increases, the tendency is for the negativistic score to decrease.

What is the possible significance of these results? True, they were obtained in an experimental situation, but it may be justifiable to assume that they give a fair representation of the reactions of the children under similar situations elsewhere. On this assumption, then, they may be interpreted to mean that in spite of the variety of treatments which the children receive, there is a tendency for them to become less negativistic as they grow older.

This comparison of the means yields a way of showing the general trends of the different age groups. All two-year-olds should not, however, be thought of as "negativistic" and all four-year-olds as "docile." There is a wide overlapping of the ranges within which the scores of the several ages fall. But there is also a considerable difference between the general trends of the different age groups.

Whether the peak of negativism comes at two years or below is a question upon which the data of this study do not throw light. The technique as devised was not applicable to the ages below two years, and it was not possible to secure comparable data on ages younger than this.

On the basis of the results of this study, it would appear that there was no waxing of negativism in the two-year interval, but only a waning. Whatever waxing there was, took place earlier than two years. Obviously, the two-year peak which this study showed may not be the real peak. It would be worth while, for instance, to study the behavior of the child from one to three years on suitable situations in order to locate the real peak of the negativism.

From the practical point of view, however, it is interesting to see from the data of this study that negativism does appear to *wane consistently* from the age of two years. It certainly seems to be a transitory stage through which the child passes, perhaps earlier than we have previously been inclined to believe. The encouraging thing is that the refusals which make it difficult to manage the young child tend to decrease as he grows older, whether any one uniform, prescribed thing is done or not. The steady effort on the part of elders to reduce this negativism is evidently successful in the majority of cases. There remains, however, a group who carry this childhood behavior into adult life. It would be interesting to know what treatment they received when they were of preschool age.

MENTAL AGE AND NEGATIVISM

Is there a similar relation between mental age and intelligence quotient and the negativistic scores obtained? These are other questions upon which the results of this study yield data.

Table III gives the figures which may indicate the answer to these questions. It will be noted that the correlation between negativistic score and mental age was —.48, which is similar to that between score and chronological age. This can also be interpreted to mean that as the mental age increases, the negativistic score tends to decrease. The correlation between negativistic score and IQ, however, is practically negligible, showing that there was no general tendency evidenced for the high IQ's to make high or low negativistic scores and likewise with the low IQ's. Knowing a child's IQ did not tell us anything about whether he was apt to be negativistic or not.

Negativism seemed to be largely a matter of chronological and mental age development. Since these two progress together, although at varying rates, it can be expected that they would correspond closely with each other in their correlation with the negativistic score. The correlation between chronological age and negativistic score is slightly higher and therefore more significant than that between mental age and score. In cases of a discrepancy between a child's chronological age and mental age, a slight tendency for him to react according to his chronological rather than to his mental age might be expected.

TABLE III

Correlations Between the Negativistic Scores and CA, MA, and IQ

Correlation	*r*	*P.E.*
Score and CA	—.53	±.03
Score and MA	—.48	±.03
Score and IQ	—.09	±.04

Partial Correlation	*r*	*P.E.*
Score and CA with MA Constant	—.29	±.04
Score and MA with CA Constant	—.12	±.04

The partial correlation between chronological age and score, with the mental age held constant, was $-.29 \pm .04$. This bore out the conclusion suggested in the preceding discussion. When mental age was partialed out, there still remained a general tendency for the negativistic score to decrease as the chronological age increased. The partial correlation between mental age and score with chronological age held constant was $-.12 \pm .04$. Again, it was found that these partial correlations tended to bear out the conclusions which were stated earlier. There appeared to be a slight general tendency for the negativistic score to decrease as the mental age increased, even when the chronological age was kept constant. Comparing these two partial correlations, it was noted that the one between score and chronological age, with mental age held constant, was higher than that between mental age and score with chronological age held constant. This gives further evidence that chronological age bears a closer relation than mental age to the negativistic tendencies shown on the experimental series.

DISCUSSION

One of the apparent causes of negativism is the conflict between the wishes of the adult and those of the child at the particular moment of the request. The growth of the child's interests in the preschool age brings him into a new conflict with the interests of adults. Negativism may not be so often a refusal for the sake of refusing as a refusal because of other interests. If we take an impartial view of the situation, it may be that sometimes it is the adult, not the child, who is negativistic.

Another cause of negativism may be the child's lack of adequate language expressions. "No!" it must be remembered, is about the only tool which the two-year-old has at his command to express his unwillingness to do as we wish him to. The four-year-old uses "Yes, I will in just a minute," "Wait 'til I get through doing this," and other politer forms, just as adults do under similar circumstances. Also, it is quite possible that the ordinary use of *not* and *can't* is not thoroughly at the command of some of these smaller children. The child who says "I can't" and then proceeds to do whatever it was he said he couldn't do, and the child who says "I'm *not* coming with you, am I?" and then trots peaceably along without more ado, may be examples of this lack of understanding of the language involved rather than instances of opposition.

The fact that the greatest amount of negativistic behavior is found with the two-year-old makes the children of three and four who exhibit it in any great amount stand out as unusual. It is the writer's opinion that the so-called clinically negativistic child of these older ages is merely exhibiting two-year-old behavior in respect to cooperating with adults; that he has failed to grow up in this respect as his contemporaries have done.

Any suggestions for managing the negativism of the preschool child must presuppose a sympathetic understanding of him as a personality. Then, on this basis, a spirit of cooperation should be established between the child and the adult. Requests of the child should be couched in the form of "Let's do this, shall we?" implying a willingness to meet him at least halfway. All but the absolutely necessary issues should be avoided, and his language refusals should not be taken too seriously. When active resistance and defiance are encountered, a break in the proceedings is more likely to produce the desired result than persistence. Give the child a chance to capitulate and still save his self-respect.

There is probably no one general rule for managing negativistic behavior. The best plan is not to provoke it, but if it is inadvertently aroused, it should be studied sympathetically and treated in the light of the special circumstances of the case.

Two—The Measurement of Nervous Habits

WILLARD C. OLSON

ABRIDGED FROM PAGES 31-41 OF THE MONOGRAPH, MINNEAPOLIS: UNIVERSITY OF MINNESOTA PRESS, 1929, BY PERMISSION OF THE AUTHOR AND THE PUBLISHER.

Attempting to record every occurrence of a given sort of behavior is often almost an impossible task. It is much easier to set up an arbitrary time unit for observation and check whether or not a specified response occurs within that period. This method is called the "time sampling technique." One of its first applications was made by Olson in the study which follows.

MATERIALS

The observer was provided with a seating chart of each room with the name of the child in each seat and squared spaces for making 20 entries below the name. Space was provided on the chart for identifying data such as grade, room number, teacher, and date, and for the time of beginning and ending each of 20 observations. An ordinary watch with a minute hand was used for recording time. A board with a clamp served for a writing surface.

METHOD

The observer took a position at the front and left (pupils' left) of the room. If the room has the proper natural lighting this position will avoid or minimize the glare of light in the observer's eyes. The unit of measure was one or more oral habits per five-minute period. Oral habits as used in the revised study included thumb-sucking, finger-sucking, nail-biting, and protruding tongue. Putting the pencil in the mouth was not counted unless accompanied by thumb or finger. Although the pencil may be a substitute for the thumb or finger in many cases and pencil-biting may be considered in part a nervous phenomenon, the inclusion of such observations would introduce an adventitious circumstance. But one entry was made per child per five-minute period, regardless of the frequency within the period, so that with 20 observations the possible range of scores was from 0 to 20. Each manifestation was recorded under the appropriate name on the chart and in the square with a number corresponding to the number of the observation. The time of beginning and ending each observation was recorded.

TIME OF OBSERVATION

In the present research all oral observations with the revised technique in the elementary grades were made in the morning at the rate of one room per morning. Observations commenced with the opening of school (9:00 A. M.) and continued for 20 five-minute periods. The observations were made consecutively except when recess or unfavorable observational conditions as defined later intervened. It is probable that data collected in the afternoon are not absolutely comparable in terms of amount to data collected in the morning, although the relative differentiation of the pupils remains the same. The single five-minute observations of oral, nasal, hirsutal, ocular, aural, and genital categories were made consecutively in the order named beginning with the opening of school in the afternoon of the same day as the 20 oral observations. An exception occurred in the two first grade rooms, which were on half-day sessions.

In the departmental grades it was necessary to confine all observations to the first hour in the morning in each room. To secure 20 observations it was necessary to observe two first hour periods. Undesirable observational conditions could not be avoided as in the elementary grades.

The observations in the present chapter were made during the month of April. It is not known to what extent seasonal variations may affect the results. Pediatricians report an apparent increase in allied disorders in the spring months.

UNDESIRABLE OBSERVATIONAL CONDITIONS

Continuous and active use of the pencil or hands under the teacher's direction inhibits the manifestation of the symptoms observed. Classes in formal penmanship, music, and drill in arithmetic were avoided, as were observations during periods when children were out of their own seats.

THE OBSERVER

Differences due to the observer have not been investigated in detail. It is probable that practice increases somewhat the accuracy of a given observer. The present data were collected after an experimental period involving not less than 12 hours of actual observation time. It is probable that a few practice periods should be spent before attempting to collect data for any serious purpose. The task itself is simple—the important thing is objectivity and accuracy on the part of the observer. The conditions have been simplified to reduce the variability of the observer to a minimum.

TABLE I

Distribution of Oral Scores by Sex for Elementary and Departmental Grades

Scores	*Elementary*			*Departmental*		
	Boys	Girls	All	Boys	Girls	All
20	2	—	2	—	—	—
19	1	7	8	2	—	2
18	3	10	13	2	2	4
17	3	14	17	4	0	4
16	9	21	30	1	2	3
15	16	15	31	3	6	9
14	13	14	27	1	3	4
13	13	10	23	1	5	6
12	14	29	43	6	7	13
11	14	17	31	3	6	9
10	15	12	27	5	6	11
9	15	21	36	4	12	16
8	13	18	31	2	5	7
7	18	16	34	7	5	12
6	17	14	31	9	4	13
5	11	9	20	9	5	14
4	16	8	24	4	5	9
3	12	6	18	8	7	15
2	10	1	11	5	3	8
1	9	—	9	5	2	7
0	1	—	1	2	1	3
N	225	242	467	83	86	169
Md.	9.3	12.0	10.7	6.9	9.5	8.5
M	9.5	11.7	10.7	8.2	9.3	8.8
S.D.$_{dis}$	4.67	4.30	4.63	5.05	4.31	4.72
S.D.$_{m}$	.31	.27	.21	.55	.46	.36

With the method and under the conditions outlined above the following data were collected.

DISTRIBUTION OF ORAL HABITS IN CHILDREN

Records were made for 467 children in grades one through six, and on 169 children in grades seven and eight. These numbers represent the entire population of an elementary school of Minneapolis with the exception of those individuals who were absent on the day of observation. The range of scores is from 0 to 20 with continuous variation between (Table I). The mean score of grades one through six is 10.7 and the mean of the departmental grades is 8.8. The means and medians agree very closely for all distributions except for the departmental boys where there is some evidence of positive skewness. The measures of variability are homogeneous. These data, as well as those presented earlier in the study, indicate that habits are distributed among children in the form of a continuous distribution. Properly stated the question concerning a child is not, "Is he nervous?" but, "How nervous is he?"

There is a difference of 1.9 in the mean manifestation in the first six grades and in the departmental grades. This difference is about 7.8 times the probable error of the difference, which indicates that it is not a chance fluctuation due to sampling. An analysis presented later reveals that this difference is not ascribable to age differences, because it is maintained when children of the same age in the two groups are compared. It will be recalled that the departmental grades were observed under different conditions than the elementary grades and this factor seems the most probable explanation of the difference. It is suggested that in careful work where children are observed under dissimilar conditions and it is necessary to make subsequent composites of the results it would be advisable to transform scores into units of the standard deviation. Each child thus receives a score that represents his deviation from the mean of the group and eliminates constant errors affecting the mean. Some such transformation scheme is especially essential when children are observed under conditions that obtain in preschools and kindergartens. The activities in which kindergarten children engage inhibit many of the movements, and successive observations during the course of a morning will result in a lower mean. If optimum conditions are chosen in these early ages, higher means will result, as illustrated elsewhere with preschool children. Under the usual classroom conditions, observations taken in each room during the morning will be comparable.

RELATIVE FREQUENCY OF VARIOUS HABITS

Single five-minute observations were made in each room for the first grades for oral, nasal, hirsutal, ocular, aural, and genital habits. This was done to establish more reliably the relative frequency of these habits and to furnish data for group comparisons.

TABLE II

RELATIVE FREQUENCY OF NERVOUS HABITS
(N = 459)

Category	*Frequency*
Oral	247
Nasal	127
Hirsutal	105
Ocular	69
Aural	48
Genital	17

While the absolute values have little significance and single observations are unreliable, the massing of such observations on large numbers of children offers a basis for group comparisons. The data obtained (Table II) indicate the same order for the frequency of these habits as was found in the preliminary study of school children.

SEX DIFFERENCES IN NERVOUS HABITS

The girls in the first six grades average 2.2 higher in oral habits than the boys. The difference is 1.1 in the departmental grades and is in the same direction. These differences are respectively, 7.9 and 2.3 times the probable error of the difference and represent a probable significance of 1,470,588,234 to 1 and 7.3 to 1. Analysis indicates that the sex differences are maintained at practically all ages.

An analysis of the data on the various nervous habits reveals that the girls exceed the boys in oral, hirsutal, and ocular habits, and that the probability of such differences being significant is high in all cases. The boys exceed the girls in nasal, aural, and genital habits, but the probability of the significance of such differences is low. The summary figures indicate a high probability that girls have more nervous habits than boys—the chances being 142 to 1. These data are in accord with those presented for the oral habits by the successive observation method and are in accord with clinical experience.

THE RELATION OF NERVOUS HABITS TO AGE

The data presented in Figure 1 indicate that age differences in the manifestation of oral habits are unimportant. On the whole the medians are remarkably homogeneous considering the size of the samples. The data for the girls are more homogeneous than those for the boys. For normative purposes it would appear that children of all ages represented may be included in single distributions when the conditions of observation have been the same. The lower manifestation for the departmental grades persists when children of the same ages are compared.

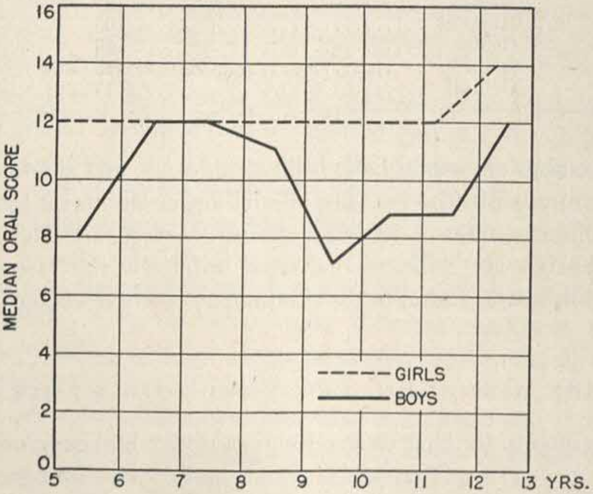

Figure 1. Age and sex differences in oral habits.

In order to get comparative data to illustrate the effect of age on oral habits a series of observations was made on a small group of University students. The students were members of a summer session class in elementary educational psychology and were, for the most part, teachers of experience. Twenty observations were made in two recitation periods on successive days beginning at eight o'clock in the morning. Four men are included in the total of 22 cases and their mean is lower than that for the women. The mean (10.7), median (10.3), and standard deviation (4.5) are practically identical with the same constants for the first six grades of the elementary school and point again to the absence of a growth cure.

SUMMARY

The results of the application of the revised techniques to an elementary school population and to a group of adults have been presented in this chapter. The data indicate that nervous habits are distributed in the same fashion as measures of mental and physical traits. The amount of nervous habits appears to be unaffected by age, but a real sex difference has been established. The girls have, in general, large amounts of the nervous habits studied. In the elementary schools a selected group of nervous habits reveals an order of frequency as follows: oral, nasal, hirsutal, ocular, aural, and genital. For an extended observational program more data can be obtained in a given amount of time by using the more frequently occurring groups of habits.

Three—Play Therapy and Race Conflict in Young Children

VIRGINIA M. AXLINE

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Free play situations are employed both to reveal personality and to assist in bringing about better adjustment. The following description of a permissive play group illustrates how a concern with racial discrimination revealed itself and was, to some extent, ameliorated by play therapy technique.

There seems to be a growing interest on the part of many people today to explore more fully and objectively the dynamics of group behavior in an attempt to determine better methods of meeting the issues that sometimes arise in the areas of human relations. Certainly the race issue is one that provokes further investigation and study. And as one sees examples of individuals becoming involved in race issues, with the scatter and spread and clash of the generated emotionalized attitudes that are often a part of this issue, many questions arise that might throw some additional light on this problem if they could be answered in such a manner that a constructive way of meeting the problems would be forthcoming.

In this article there is a brief account of some experimental work done in group play therapy with six-, seven-, and eight-year-old children. The primary purpose of the study was to determine the effectiveness of play therapy for small groups of children who were having difficulty adjusting to other children. The children who were selected for participation in the group therapy experience were either extremely withdrawn or aggressively antisocial. There were four groups of children from four different classrooms—and four children in each group, two girls and two boys.

The plan for the therapy was as follows. Each group would meet with the play therapist for one 40-minute period each week for 10 meetings. At the conclusion of the tenth meeting the groupings would become transitory, mixed groupings. In other words, the eleventh meeting of the grouping would be composed of four children, one from each classroom represented in the study. The next four meetings would continue on this pattern so that each time there would be a group of children who had never been together in the play group before. The purpose of this procedure was to study the children's adjustability to new group experiences.

A study of the data of this experiment revealed some interesting material on the problem of racial conflict among young children and the results of the group experience for these children. In this article only that material related to the race issue will be included.

This material is presented here for consideration as a possible means of studying the dynamics of group behavior, and the generation and dissipation of the childrens' emotionalized attitudes. It is presented with these questions: Does this type of verbatim descriptive material seem to lend itself to a scientific study of the "race issue"? And does this therapeutic procedure seem to be an effective way of resolving racial conflict among children?

These play contacts were not directed by the therapist. The children were in a free play situation where they could express any attitudes and feelings that they wished to express. In this type of experience we often see the child's real self in spontaneous expression. Consequently, it seems that such material might offer rich resources for a detailed study. These excerpts, lifted out of the complete context of the record of the childrens' play, are presented only as examples of the type of behavior that is available for further study and analysis. Brief as they are, however, it seems to me that they suggest a way of studying the race issue from the inner frame of reference of the child. It seems as though a more detailed collection of data, a more rigorous analysis of the material, and a more systematic procedure might yield some very significant studies of the race issue.

In the nondirective play therapy sessions no attempt is made to bring out any specific type of material. Consequently, the examples offered, coming as a completely spontaneous expression on the part of these children, seem to be more valuable because of that very spontaneity.

When the complete record of this play group is studied one notes that these children are not always "race-conscious" children. Nor does the issue loom up after every clash of personalities. One might ask: Why does it become an issue at one time and remain completely out of the picture at another time? The play experiences of these children ran the gamut of the usual group therapy play with all shades of interaction and self-expression coming out. In only five brief incidents was there any obvious awareness of the race issue on the part of these children. So, when studying these children's behavior and emotional reactions, it seems well to keep this in mind so that they are seen in more accurate perspective.

In the first group mentioned are four seven-year-old children. There is June, a Negro. Her teacher describes her behavior as "aggressive, quarrelsome, sulky. The children do not like her because she is always starting trouble." Beverly's teacher describes her as "wild, rough, loud, aggressive, destructive and *naturally* not liked by the other children." Jackie is described as a "very quiet, dreamy, child, who will not participate in group activities, but who always clings to any adult in the group." The children do not dislike Jackie, according to his teacher, but he rejects any overture from them to include him in the group. Pete is described as "*impossibly* aggressive, *highly* emotional, *extremely* quarrelsome."

This constitutes the membership of the first group. The first and second meetings were stormy ones—with the children frequently arguing over the toys—usually playing alone. Jackie, true to his reputation, assumed the role of kibitzer most of the time. During *the third meeting* of this group the following incident occurred.

June's opening remark as she enters the room is to the effect that she had gotten sand in her hair the last time she had come to the group meeting and her mother had said that she was not to get sand in it again. Beverly, who is loud, active, aggressive, jeers at June. "So what if you do get sand in your hair? You know what our rules are. We can throw sand in this half of the room. And if you come over that line you risk getting sand in your hair. And if you don't like it you should stay over there on that side and not come over here and try and bring the rules for that side of the line over here on this side of the line."

Jackie is a quiet, gentle dreamer who talks a great deal to the therapist but has up to now stayed out of any active group participation—other than calling across an observation to one of the other children.

Pete is active, excitable, and aggressive. He wants to run the show. When things do not go as he wants them to go he quickly doubles up his fists and asks belligerently, "Do I have to *show* you I mean business?"

June and Beverly went into the puppet theater—June crossing the line and squealing out, "Don't throw the sand on me!" and the boys throwing it because she stepped over the line. Finally they settled down. The girls idly manipulated the puppets. The boys played in the sand. Then the girls went over to the easel and began to paint. Pete commented that he didn't see why they couldn't do absolutely everything they wanted to do in this place even to killing off one another and demolishing the room. He had experienced the limitations only when he had in previous contacts made an attempt to strike someone in the group and when he had not been permitted to throw paint on the walls or to dig off the plaster. He had accepted the limitations but was still verbally growling about it. The therapist accepted his feelings of wanting to do these things, but held the limitations firmly in place.

Suddenly the two boys whispered together, then quickly jumped out of the sand box and proceeded to wreck the collapsible doll house. This was an acceptable play of destructiveness, the house having been constructed for that purpose. They screamed and yelled.

Pete: "Tear down the house. Wreck it."

Jackie: "The morons will completely demolish this dump."

Pete: "Off with the roof. Off with the walls. I hate—hate—hate this miserable house. The walls close in on them all. Bang, goes the roof right smack down on them."

Jackie: "Did they get killed? Did they? Did they?"

Pete: "Of *course* they got killed. They died *horrible* deaths. And their blood ran like rivers down the street."

The two girls were watching the boys. Beverly picked up her brush, dipped it into the red paint, and swiped it across the picture.

"Blood—blood—blood!" she yelled with vigor. "All over everybody and everything. Come on, June. Let's let them throw the sand at us."

"All right—all right!" June cried—and they leaped quite happily across the "safety" line. Immediately the boys grabbed up handfulls of sand and threw it at the girls. Immediately June reverted to her usual protest and Beverly as usual pointed out to June that she could not come over the line unless she would take the consequences. It was all so noisy and moved so rapidly that the therapist said nothing until a remark was addressed to her. "People are so very funny," Jackie said to the therapist. "They pretend they do not want the thing they want the most. June *wants* us to throw sand at her. She yells when we do. I *say* I don't like noise and fights and I could scream all day long and stick knives in people."

"People don't always behave the way they feel, hm?" said the therapist. "You act and say one thing and sometimes mean another?"

"Yes," Jackie said. "Quite often I do. But I never tell how I *really* feel. I *pretend—pretend* all the time."

June and Beverly lay down on the floor and rolled. Pete dumped large boxes of sand on them. They laughed and picked up handfulls and threw it back at the boys.

Then June got in the sand box. There was an immediate protest. Both boys declared the sand box was their territory and called the girls invaders.

The girls retreated to the puppet theater and put on a play. The boys watched. All the boy puppets were beaten up by the girls—declared "incompetent, feeble-minded and morons." Beverly then announced that she *really* loved boys and wished she was a boy. Jackie said he *loved* girls and wished he was a girl. Pete said he was happy just the way he was—but wished he was a king and that everyone *had* to obey him. June said nothing. She withdrew to the easel. Beverly's loud voice demanded that June, too, should say how she felt about herself. June turned her back to the others. Beverly continued to demand an answer. June turned defiantly and said, "I will *not* tell you. And you *white folks* went ahead and got sand in my hair *again.* And I *told* you not to."

"It was your own fault," Beverly shouted. "You got over the line."

Angrily June stepped across the line again. "I'll come across the line again," she declared, "and again and again and I'll tell my mother you *white folks* did this to me."

Because she was across the line, the "white folks" again threw the sand at her. The therapist reflected the attitudes all the children expressed. She did not attempt to restrain the sand-throwing because June was over the line. When June got back in the safety zone and Beverly threw more sand for good measure, then the therapist entered in with a limitation. "This is the safety zone. No sand over here."

June sulked. She returned to the easel and stirred the paints without much interest in what she was doing.

Suddenly June turned toward the others, who had quieted down and were now playing together in the sand box.

"Look at you three together," she cried. "You awful three! You white trash!"

Beverly sprang up, "Don't you call me that—you—you—" She looked furtively at the therapist.

"You want to call her a name because she called you one, don't you?" the therapist said.

"Yes," Beverly replied.

But Jackie, the young philosopher, said, "Be kind to her, Beverly. Her feelings *must* be hurt." He dug quietly in the sand—looked up at Beverly and said, "You really wanted to call her a nigger, didn't you?"

Beverly hung her head. She did not reply. June looked at them with hot eyes.

"Do you want to come in here and play with us?" Jackie invited June.

"No," June said.

"Come on," Jackie said.

"Well," said June.

"You can have my place," Jackie said, jumping out, "and I'll paint."

June gingerly took Jackie's place. Beverly handed her a shovel and a little bucket. Pete retreated to a corner in the sand box with his back to the girls. Jackie painted a piece of paper solid black. Then he placed a thick gob of red finger-paint in the middle. He came over to the therapist and whispered:

"Guess what it is?"

"I don't know," said the therapist. "Do you want to tell me?"

Jackie grinned wickedly. "It's got something to do with June," he whispered, "and it *looks* like she got hurt."

Beverly and June started to play together and soon Pete joined them. Finally Jackie came over and sat on the edge of the sand box.

"Do you like us better now?" he asked June.

She smiled at him quite happily. "Uh-huh!" she said.

Jackie went over to the easel and tore down his painting and threw it in the waste basket. He looked at June.

"What color do you like the best?" he said.

"Pink," she said.

Jackie carefully mixed some red in the pan of white paint. He tried it out on the paper.

"This pink?" he asked.

"Yes," she said and settled back to watch him. He painted a row of pink flowers, added green stems and green leaves.

"There," he said as he finished it. "This is for you, June."

"Make me one! Make me one!" called Beverly.

"No, that's all I want to do today," Jackie said.

"Our time is up for today," said the therapist.

June chose that minute to pour a handful of sand on Beverly's hair, as she jumped out of the sand box. There was a squabble and a flare-up. Finally they quieted down and left together to return to their class at school.

This is the kind of data that seems to contain valuable material for a study of attitudes and behavior of children. A few of the significant things that stand out are the comments of Jackie, implying he does one thing, feels another, "pretends, pretends all the time." The way June reacts when they ask her how she feels about herself is interesting. The injection of racial feelings and the way in which the group meets this challenge seems important. Does this seem to be significant material for a study in attacking the problems related to social issues?

Following this meeting it was necessary to place June in another group. This was due to the fact that she was transferred to another classroom and the teacher's reading schedule conflicted with the time of the group meeting. June was placed in Group 2. *This was the fourth meeting* of this group, the fourth group therapy meeting for June, but her first meeting with these children. The children were well acquainted, however, outside of the group.

These children are also seven years old. They were referred for the group therapy experience for the following reasons, as stated by their teachers:

Louise is very quiet, very withdrawn. She seldom plays with the group—seldom asserts herself.

Perry is inclined to be domineering and bossy. He always likes to have his way. He is excitable and nervous. He does not get along with other children—does not seem to want to.

Rollin is moody—at times very withdrawn and again very aggressive. He is a lone wolf most of the time.

Louise did not live up to her teacher's description at any time during the therapy. Perry and Rollin behaved more or less as they had been described.

June was the only Negro in this class at school. An excerpt from the group which June joined illustrates a little more how the child sometimes expresses significant attitudes in his play—and how these attitudes are influenced by other children in the group.

Rollin, June, Louise, and Perry are playing together in the sand for the first few minutes. Then the group splits up. Rollin and June play at the doll house. Perry and Louise paint. The play at the doll house centers around the dolls going to the toilet.

"Look at him. He's on the toilet," Rollin says.

"Shame! Shame!" June cries.

"Why?" Rollin asks.

"I don't know," June says.

Then they set the table and begin to play house.

"We're eating breakfast. This is fun," June cries.

They play around. June bumps into the table and knocks it over.

"These children are bad girls!" Rollin shouts.

"They are *not* bad," June shouts in reply.

"You *dope*!" Rollin yells.

"Who says I'm a dope? I'm *not* a dope. You're a dope yourself!"

"You—you *red head* you!" Rollin screams.

"Red head yourself!" screams June.

"You should go away to the nuthead house!" Rollin says. "Come on, let's fix this so the people won't have any home to live in."

June and Rollin yell and scream and wreck the house.

June grabs up the baby doll and swings it around by the leg. Then she says, "Oh let's play right. Let's don't be so wild."

"Okay, okay," says Rollin. Let's use this big doll." He tosses it in the sand box. "I'm going to put all the people in the attic and I'll lock 'em up there and they will never in the world get out again."

The other two children continue to paint—talking quietly. Perry asks for more paper. He looks at the therapist.

"What is your real name?" he asks. She tells him her name. As he tries to tack up the paper he sticks his finger. "Look," he says, "I'll need first aid. I'll wash it off." He spills some water on the floor.

"Oh, I *am* sorry," he says.

"Why did you say you were sorry?" Louise asks.

"Well, it was an accident," Perry comments.

"I daresay you won't need to say 'I'm sorry' in here," Louise observes.

"Then that is why I will say it in here. I get so tired of saying what I'm *supposed* to say."

"Is anyone interested in modern art?" Louise asks with a giggle. "I think I've just done some very, very modern art."

Perry holds up the brown paint water. "Nice whiskey," he says.

June turns and holds up a small table from the doll house. "This is a cocktail table." As she reaches over she jars Rollin's arm and he spills the tray on which he was carrying "the drinks."

"Oh, for Christ sake," he shouts, "you Goddam nigger. You *spilled* the tray! This makes me so Goddam mad I could spit on you!"

June draws back. Her face clouds over. "I am *not* a nigger!" she shouts.

"What are you then?" Rollin demands.

June looks about her unhappily. "I—I—I am a *person*!" she says.

There is absolute silence in the playroom. All three children turn and look at June.

"Oh," says Rollin. There is another silence. Rollin looks down at the floor. He goes over and sits down on the edge of the sand box. He puts his hand down in the sand, idly sifts it through his fingers. June stands still in the middle of the room, staring at Rollin. He looks up at her again. "I'm sorry, June," he says finally.

"That's all right," June says. There is once more quietness in the room. June turns her back to the others—goes over to the doll house things and sorts through them until she finds a little Negro doll. She holds it in her hand and looks at it. Rollin still sits on the edge of the sand box watching June with an odd expression on his face.

June picks up a white doll, holds the white doll and the Negro doll side by side, looks at them for a long time. She picks up one of the doll house beds and places the white doll in it very carefully. Then she glares at the Negro doll. She lays it on the table, grabs the wooden hammer and pounds the doll viciously.

"Get rid of the old nigger!" she shouts. "Dirty old nigger. Black, hateful old nigger."

Rollin stands up quickly and looks at her. "June!" he says, "June! She is a *person*!"

"Oh," says June in a tone of distress, "I'm sorry." She picks up the Negro doll. She looks at Rollin again. "Could I—" she asks, then hesitates.

"What!" Rollin asks.

"Could I put her in the same bed with the—the *pretty* doll?"

Rollin comes over and looks at the white doll in the little bed and at the Negro doll in June's little brown hand. He considers it for a long time. Perry and Louise leave their painting and come over, too. They all gather around June, who is still holding the little black doll.

"Is there—is there another bed?" Perry asks. "Everyone should sleep in a bed of his own. No two people should *ever* sleep together."

"I sleep with my sister," Louise says, "and that's all right!"

Rollin stoops down and sorts through the toy furniture. He finds another bed. June watches him silently. There are tears in her eyes. They are not doing this to the doll. They are doing this to her. The therapist stays out of it at this point—watching this stark drama unfold slowly and with intense meaning for all these children.

"Here is another bed," Rollin says to June. She stretches the doll out toward him. Rollins does not touch the doll.

Perry reaches out a hand to take the bed. Rollin pulls it out of his reach. He glares at Perry. "You keep outa this," he says roughly.

Then he looks at June and asks her very gently, "Where does she want to sleep, June? Does she want a bed all her own? Or does she want to sleep with the white girl?"

June will not commit herself. She blinks back her tears and continues to hold the Negro doll out to Rollin. Finally he takes it, places it in the bed alone, quickly removes it, throws the empty bed across the room with violence and places the Negro doll in the bed with the white one. June smiles radiantly.

"Why did you do *that*?" Perry asks.

"I know why," Louise says. "I'm glad you did, Rollin."

"Why *did* you?" Perry demands again.

Rollin shrugs his shoulders and sits down on the edge of the sand box.

"I know why he did," Louise says. "You didn't want June to cry. Isn't that why?"

"No," Rollin says, shaking his head.

"Well, I think you're crazy," Perry says, and goes after the other doll bed. He brings it back and reaches for the Negro doll. Rollin grabs his arm.

"You let that alone!" he yells. "You keep your hands off."

"They should each have their own bed!" Perry shouts.

"Let them alone," Rollin shouts. "Let them alone!"

The group splits up again. Rollin crawls into the sand box and sits with his back to the others. June sits down at the table and rolls a ball of clay in her hands. Louise and Perry return to their painting. None of the children speaks. When it is time to go they leave quietly. June walks back to the schoolroom with Perry and Louise and they talk happily together. Rollin walks back alone—hanging his head. What is he thinking? Why did he react like that? Rollin is only eight. He is a Jew. Does this suggest that there is value in such a group for the handling of social conflicts? Does this suggest that the attitudes of even young children soon are influenced by prejudice and discrimination?

I believe that this little experience and many other group experiences will shed some light on the issue of social therapy—or social education, call it what you will. I believe this is of special significance to teachers as well as therapists.

June attended six more meetings with this group. And threee more times some aspect of the race problem was brought into focus.

The causes of the race question's being brought into the group play seem to be significant. In the first excerpt quoted here it is a bit difficult to pin down the exact cause of June's withdrawal from the group and then her attack upon them. One might speculate that she withdrew when asked to state her wish because it pointed up a difference between herself and the others—a difference which she was reluctant to face; and consequently she met it with an attack upon the other members of the group. In the second example the race issue was injected as a result of Rollin's anger. It was the climax of milder name-calling. At no time after this incident did Rollin ever bring out any attack upon June's race. However, Perry and Louise did.

The other three incidents will now be cited:

At the fifth meeting of this second group the children were playing quietly—each one pursuing his own interests. Rollin was playing in the sand. Perry was modeling an animal out of clay. Louise was painting. June was playing with the large rag doll. Suddenly June threw down the doll and sat down at the table across from Perry. He did not pay any attention to her. She reached over and took a piece of his clay. Immediately Perry sprang to his feet screaming, "Give me back my clay!"

"I want it!" June said, hugging it tightly.

"Give me it! Give me it!" yelled Perry angrily.

He reached across and snatched at it. June held it in her hands, sat down on the chair, bent over so that Perry could not get the clay out of her hands.

Perry was beside himself with anger. The therapist reflected the attitudes the children were expressing—June's desire to keep the clay and Perry's anger at the act. June grinned at the therapist. "He's sure mad!" she said gleefully. "Look at his face. He gets so red in the face when he gets mad," she giggled. "I *like* to make his face change color!"

Perry glared at her and said coldly, "I'd like to make *your* face change color. I'd like to make it *white* so you wouldn't be so mean!"

The therapist said, "You would *both* like to change the color of the other's face?"

June stood up and hurled the clay down on the floor as hard as she could.

"Don't you call me names!" she yelled at Perry.

"June doesn't like to have someone call her names," said the therapist.

Perry looked angrily at the therapist.

"She is a mean, nasty—"

"I am *not*! I am *not*!" screamed June and tears rolled down her face.

"Look," Perry said, suddenly speaking in a very calm voice, "I didn't *like* it, June, when you jerked away my clay. Why did you do it to me?"

"I dunno," June said meekly. She hung her head, wiped her eyes with her fists.

Louise came up to June. "Do you want to play with one of us?" she asked. June nodded.

"Want to paint with me?" June nodded.

Louise looked at June and suddenly reached out and laid her hand on June's hair.

"Your hair feels funny," Louise said. June put her hand up to her hair. She looked at Louise suspiciously. Then she reached out and touched Louise's hair.

"*Your* hair feels funny," June said belligerently. But Louise was not making fun of June. She was interested in June's hair and touched it gently with both hands.

"Your hair is *different*," Louise said.

"*No*!" June protested, burying her hands not so gently in Louise's hair.

"*No*! It's your hair that's different."

"You've *both* got *different* kinds of hair," said the therapist.

Rollin got out of the sand and came over and touched June's hair very gently.

"I think your hair is *nice*, June," he said.

Perry stuck his hand on June's hair, then on Louise's hair, then on his own hair, then on Rollin's hair.

"Your hair is sort of like June's," he said to Rollin.

"Is it?" June asked. She felt Rollin's hair. He shook off her hand and got back in the sand box and kept his back to the others.

"Each one of you has hair that is a little different from one another," said the therapist.

June laughed. Louise laughed. Perry laughed.

"But it *all* grows on our heads!" Perry said. He sat down at the table. "Here's some clay if you want it," he said. June sat down and played with the clay.

Here again we note the attack, the anger, the hurt feelings when June is made to feel "different." Here again we see the dissipation of the feeling when June does not feel alone in her "differences." The exploratory interest all the children displayed as they studied the differences in the texture of their hair brought with it the calm of a shared interest. Perry's generalization that they all had one thing in common—it grew on their heads—seemed to dissolve the last shred of tension between them.

At the sixth meeting of the group Louise was painting a picture of a "princess." It was a very good picture for a seven-year-old child to draw. June watched Louise. Rollin came over and looked at it. "That's pretty," he said, with genuine appreciation of Louise's art.

"I don't think it is," June said. Rollin walked away from the easel. June picked up a brush, dipped it in the brown paint, and smeared it across the face of the "princess." Louise turned on her angrily. "You look what you've done!" she cried. "You've made my beautiful princess into an ugly nigger!"

June glared at Louise.

"Louise is angry because June *painted on her* picture," the therapist said.

June stood still and looked at the painting, then at Louise, then at the therapist.

"She called me a nigger," she said, but her voice was quiet.

"I did *not* call *you* a nigger," Louise said, "I said you spoiled my picture by putting that paint on it."

June looked again at the picture and then at the therapist.

"I guess I *did* spoil the picture," she said, "I'm sorry."

In this incident June for the first time seemed to realize that she was partly responsible for the attacks, she drew upon herself. The fact that June had used brown paint and had smeared it on the face of Louise's "princess" seemed to have suggested the accusation Louise hurled at June. Why did June use the brown paint and smear it over the face? I don't know. The therapist's statement seemed to point out to June the fact that it was the *paint on the picture* to which Louise was reacting, rather than the color of June's skin.

During the seventh meeting the fifth and last incident in which the race issue was brought out in this play group occurred in a very interesting manner. Rollin was finger-painting. Three of the children were playing "house." Perry said he would be the "father" and Louise would be the "mother" and June would be their "baby." Thus they started their play.

Suddenly, without any obvious reason for his change in attitude, Perry announced that June could not be their baby any more. She would have to be the maid.

"Here, Black Girl," he said "Go get–"

That was all that it took to set June off.

"I am *not* a black girl," she yelled.

"I didn't mean to make you mad," Perry said. "*I* wouldn't get mad if you called *me White Boy*."

June looked at him, bewildered by what Perry had said.

"But she isn't *black*," Louise said. She went over to the table where Rollin was finger-painting. She brought back the jar of brown finger-paint and took off the lid.

"Look," she said, "This is just the color of June." She dug out some of the paint and held it toward June's arm. At first June drew back, then, noting that Louise was not attacking her or making fun of her, she looked in the jar. Then Louise rubbed the brown finger-paint on her own hands and arms. Perry, rising to the occasion, rubbed it on his hands and arms and face. Then Louise put some on her face. They were all laughing. June reached in the jar and rubbed the paint on her arms and hands and face.

"Look!" Perry shouted. "It *matches* exactly."

Rollin stopped his finger-painting and sat there grinning at them. Suddenly he joined the others and smeared the brown paint on his hands, arms, and face.

"This is fun! This is fun!" they cried dancing around the room.

"Where's some *white* finger-paint?" June asked. They all stopped and looked around for the white finger-paint.

"There isn't any white finger-paint," said the therapist.

"We're all *alike* now," Louise said. "June and Perry and Rollin and me!"

And the children danced around smearing the paint on themselves for the remaining ten minutes of the play period. When it was time to leave they all washed their hands and arms and faces and left together in a happy, cheerful mood.

After this session there were no other incidents that referred to June as a Negro—or as anyone differing from the group. They all seemed to have accepted one another. This also carried over in the classroom situation. The teacher's report states that June has become "more sociable, more cooperative, more friendly, with a definite decrease in her former aggressiveness." The report also notes that June was accepted in her classroom by the other three children in her therapy group—and gradually by the other children in the class.

The report in regard to the other three children mentions the following changes:

> Louise has changed more than any of the other children in the group. She is now definitely a leader of a group and gets along well with the other children.
>
> Perry is much more agreeable. He still likes to boss the others around but does not react unfavorably when put in his place by one of the others. He plays more with the other children.
>
> Rollin is still moody—but seems to have more *ups* than downs. He seems quieter, more relaxed, less tense. He plays quite a bit with his "play group."

These children made interesting adjustments to the last series of group meetings. There was a tendency to participate in the group meetings with an awareness of the rights of others. There was a marked tendency to plan what they would do at the beginning of the meetings and the plans were usually carried out.

Since this occurred in all the meetings, it seems that one could infer that these children had learned how to adjust to others in a free play situation so that the rights of others were considered. Any clashes that did occur were gotten out in the open immediately and some satisfactory solution was arrived at by the children. Sometimes they each played alone, but most of the time they played together. There was a considerable lessening of destructive, aggressive play as the time passed. Finally, and certainly important, at no time during these last five meetings did the race problem become an issue. It seems to have disappeared from the attitudes and feelings of these children.

These children seem to have been able to get beneath the surface and to have achieved a respect for the personality of one another, an acceptance of their differences, and a perception of a common bond between them.

When one provides a situation wherein the children are given an opportunity to be themselves—and an opportunity to interact in a very permissive situation, then it seems that they can more readily come to terms with their own attitudes and emotions; and in a face-to-face situation where their free expression is *not* checked, they *can* and *do* assume responsibility for their attitudes and can experience the effect that the emotional expression of those attitudes can have upon themselves and others.

So it seems that these small group experiences had special significance for these children and enabled them to offer emotional hospitality and understanding to one another that seem to be necessary for us if we are ever to achieve a togetherness of effort and a unity of civilized thought.

Four—Shirley's Babies After Fifteen Years

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In Chapter II, some account was given of Shirley's study of 25 children during the first two years. Neilon, who studied the same individuals at the age of seventeen, has made a unique contribution to the study of constancy of personality.

The matching technique that she used does not show what aspects of personality remain constant but shows that there is enough in common between two personality descriptions of each of a series of persons to permit correct matchings at better than a chance level.

Although mothers, nurses, and other observers of children have often been convinced that there is some continuity of "characteristicness," or general personality pattern in developing individuals, this continuity has been elusive of objective psychological measurement. That individuality exists in early infancy has been demonstrated by several investigators. That it also exists in adulthood is, of course, accepted, and differences are measured. That the pattern of personality shown in infancy continues through life, or that it changes its form of expression as growth and development proceed has, however, never been established. Just as manifestations of intelligence change with age, manifestations of individuality might change with development, while leaving the child in the same relative place in his group at successive measurements. Even assuming continuity of personality patterns, the individual who cries a great deal as an infant would not necessarily be expected to cry a great deal as an adult. The personality characteristic which caused the excessive crying in infancy might persist though, and be expressed in a different manner in adulthood. We do not know merely from the infant's crying, however, what form of expression this characteristic might take later. The infant who cries a great deal might babble a great deal as a toddler, and be talkative as an adult. Or, the same infant who cries a great deal might be subject to severe temper tantrums at preschool age, and have an inclination to impulsiveness or emotionality in adulthood. The entire personality pattern, including emphases and details might continue from infancy with changes in expression to adulthood. Or, perhaps, some elements of individuality might be retained while others disappeared with development and experience. Or, there is a third possibility that differences which existed in infancy were perhaps due to differences in age at birth, and to physical and physiological conditions. Although these and other more complex possibilities unquestionably exist, they have not been investigated. In part this is due to the difficulties of measurement in the field of personality, and in part to the lack of attempts at measurement of the same individuals after long intervals of time have elapsed. Moreover, measurements which are suitable for infants or children are inapplicable to adults and *vice versa*, so that continuity of measurements is impossible. As a result, most students of child personality have used the cross-sectional approach to investigate age and sex differences in a certain trait, or to produce experimental modifications. Rarely have they attempted to measure continuity in the natural environment.

PREVIOUS STUDIES

Early biographers seem to have believed in the emergence of individuality in infancy. Shinn (22), for example, says: "Our baby showed temperament and luckily of the easy-going and cheerful kind from her first day (though we could hardly see this except by looking back afterward)." Hogan (11) refers to the child's "early inclination to know what was going on about him," which continued, as did his seeming "happiest when he was let alone."

In the more recent writings, evidence concerning continuity of individuality is given firstly in studies of certain personality traits, secondly in studies using personality tests and rating scales, and thirdly in studies which aim at total personality investigation, usually by use of a descriptive technique. The individual's characteristic modes of response have tended to persist in the areas of perseveration, which was investigated by Cushing (6), laughter, which was investigated by Brackett (3), and crying, which was investigated by Bayley (2). After studying "problem" behavior in a large and representative sample, however, Macfarlane (15) concluded that "transitoriness" tended to be the rule rather than "persistence." Using time sampling techniques in studying preschool children, individual consistency has been found by Green (10) in "frequency of group play," by Loomis (14) in the ratio of number of contacts initiated to those received, and by Jersild and Markey (13) in conflict behavior in two observation periods separated by a year's time interval. Because they demonstrate some degree of stability in personality, reports of continuity of such traits or behavior items are valuable. The question of the continuity of total personality pattern, however, is far more complex. While the above reports are objective, they are, for our purposes, limited in scope. Attempts to investigate "total personality," on the other hand, often sacrifice objectivity to completeness.

Halfway between studies of single traits and studies using descriptive methods to investigate total personality are studies of the continuity of individuality which use standard personality tests and rating scales as measuring devices. Test-retest techniques used by Jones and Burks (14) have shown that children tend to say the same sorts of things about themselves on two tests separated in time. While personality tests are the most objective of the measures available, such studies cannot adequately demonstrate the continuity or lack of continuity of individuality. In the first place, test-retest reliability, deliberately made a part of the test, might insure measured continuity. To many, in the second place, a person's impression of himself is not so important an aspect of personality as is the impression of him gained by others, which is not measured by a personality test. Finally, a test is also limiting, and totality is again sacrificed to objectivity. The rating scale, which is a semiobjective measure, has also been applied to this problem. Bonham and Sargent (18) used the method on 38 children from birth to two and one-half years finding no consistently positive relationships in ratings except for good looks. In *Biographies of Child Development,* Gesell and others (7) conclude:

> Our data [from 10 years of study at the Yale clinic] do not lend support to the concept of a relatively standard pattern of infancy. Nor are the findings of embryology in harmony with such a concept. From the standpoint of embryology the infant is already far advanced in the cycle of life. He is already stamped with individuality rather than with a standard pattern. . . . This perpetuation of characteristicness is not incompatible with morphogenesis and maturing. It is, however, inconsistent with the idea that individual differences at birth are slight and increase with age, or that the period of infancy is in any sense neutral or generic when compared with later periods of the life cycle.

There seemed to have been persistent temperamental differences in twins *T* and *C* (8), and Johnny and Jimmy (16). After a series of studies of the same group of children for five years at Columbia University (12, 17), "consistency," it was concluded, "rather than inconsistency is characteristic of development." Also using a descriptive technique, Allport (1) studied is own son. Here personality predictions made by the parents when the boy was four months old were compared with records of parents at two later ages and with records of four different teachers at succeeding age levels. "The prognosis at the age of four months," Allport declares, "is borne out in most respects . . . two of the initially dominant characteristics have shifted their emphasis . . . . But on the whole the schedule is consistent throughout," From this material, Allport advances the hypothesis that "from early infancy there is a consistency in the development of personality." Roberts and Fleming (19), with data from 25 women at precollege, college, and postcollege levels found that the ratio of persisting to fluctuating traits was 3:2.

Elimination of the experimenter's bias, which is a factor in all biographical studies, is achieved by Gesell (7). Movies of five children at ages one and five were used. From the movies, a trained observer who was unacquainted with the children ranked them on 15 behavior traits including energy, demeanor, dependence, social responsiveness, and the like. Out of 75 rankings, 48 coincided, 21 were displaced by one rank order, 5 were displaced by two rank orders, and 1 was displaced by three rank orders. "Our periodic cinema records," Gesell writes, "clearly show prophetic characters in behavior traits in the first year."

In the investigation of continuity of individuality, in summary, positive findings are more conclusive than negative ones. An impression of continuity was reported by early biographers. Continuity is likewise found in certain separate traits and behavior items. Investigators of "total personality" have been unable to retain both completeness and objectivity, between which a balance must be struck. Since a breaking down of "totality" has usually given negative results, it would seem that strict quantitative measurement of behavior must be discarded in this area. The method of the Gesell study (7) provides another sort of objectivity. The matching method advocated by Vernon (25, 26, 27) allows the use of total personality, but eliminates the strong effect which the bias of the single experimenter exerts upon the results in the case study or descriptive method. In one study reported by Vernon (25), character sketches of 25 subjects were written by each of 3 experimenters on the basis of the behavior of the subjects during performance tests. Each experimenter then tried to identify the sketches of the other two. This method applied to growing children should provide evidence about the continuity of individuality.

THE PRESENT METHOD

This is essentially the method adopted in this study. Extensive personality data on infants were presented by Dr. Mary Shirley in her *The First Two Years: a Study of Twenty-Five Babies* (24). After two years of standardized observations of the children as well as two years' acquaintance with them and their families, Dr. Shirley wrote personality sketches. These sketches and later sketches of the same children prepared by the writer were used in a matching procedure to investigate the continuity of individuality.

The plan for the personality follow-up of the 25 babies, which was begun 15 years after the original study, included the objective measurements of the subjects who would be available for study, and the writing of new personality sketches which were to be matched with the original sketches written by Dr. Shirley. The writer did not consult the Shirley sketches after the study was begun, and did not know the pseudonyms used by Shirley until the follow-up sketches were completed. Two formal personality tests were used in the follow-up, the Goodenough *Speed of Association Test* (9) and the Rundquist-Sletto *Minnesota Survey of Opinions* (20). In addition to these formally developed scales of personality measurement, a 5-point rating scale of 23 traits, and a scale of 6 special abilities were also used. *The Speed of Association Test, The Minnesota Survey of Opinions,* and the scale for self-rating were administered to the subject in this order. Following this was a more or less standardized interview concerning the subject's interests, in which a general impression of his personality picture could be gained. An interview was also held with each of the mothers. During this interview, the mother rated her child on the 23 traits and 6 special abilities. She was also encouraged to talk about the child, and to state why she placed him in each of the rating categories. Many anecdotes illustrating the various characteristics of the child were related, and his place in the family group was estimated. Each mother was also asked whether or not she believed the child to have changed in such respects as were referred to on the rating scale or in his general behavior pattern. Rating scales and special ability scales were mailed to the fathers. All of the objective data were interpreted in the light of the short interviews with the subjects and mothers, and personality sketches were written of the children as they seemed to be at the age of seventeen.

The original group of 25 babies was above average in socioeconomic status, education, and intelligence. Such a superior sample was originally chosen by Dr. Shirley because it was felt that better cooperation of parents could be obtained than would have been the case if a sample more representative of the population at large had been chosen. At the end of two years, Shirley had 19 of her original 25 subjects. Fifteen years later, it was possible to gather partial data on all of these, and full data on 16.

The finished adolescent sketches were two to four typewritten pages in length. To the Shirley infant sketches proper, material was added from her sections of "Incidental Reactions" and "Personality as Revealed in Speech." This made the infant sketches comparable in length to the adolescent sketches. Since sex was readily determined in all sketches, the sexes were separated for the matching procedure. There were 5 sketches of adolescent girls to be matched with 6 infant sketches, and 10 sketches of adolescent boys to be matched with 13 infant sketches. One of the adolescent boy sketches had to be thrown out because there was no comparable infant sketch. Keeping the extra infant sketches made matching more difficult by eliminating the possibility of automatically matching the last sketch in either series. Because of the length of the sketches, the matching task was a difficult one. It was performed by graduate students and staff members at the Institute of Child Welfare, University of Minnesota, who carefully read and reread the material, weighed and reevaluated the evidence. Ten judges matched the sketches of girls (5 versus 6 cases), and five judges matched the sketches of boys (10 versus 13 cases). Although statistically the chances for success are approximately equal for both tasks, the matching of 10 versus 13 cases is psychologically far more difficult. The greater number of cases necessitated more than twice the amount of reading, and made an almost impossible demand upon the memories of the judges. With 10 personality sketches and 10 pictures Vernon (27) found matching "almost impossible" for judges, even though this would involve less reading than the matching of two sets of sketches.

Chances of successful choice are both reduced and increased by the limitations of the matching process. Taking the simpler five versus six matching as an example, the chances that the first adolescent girl's sketch would be correctly matched would be one in six. Assuming this first match to be correct, the chances that the second sketch would be correctly matched would be reduced to one in five. And assuming the first two to be correctly matched, there would be one chance in four that a third would also be successfully matched, and so on, the task being made easier by process of elimination after every successful matching. The chances of matching all sketches successfully, therefore, would be $\frac{1}{6}$! When there are incorrect matches, however, the problem becomes more complex. If the first adolescent girl's sketch were incorrectly matched, it might be matched with either the sixth infant sketch, which has no mate, or with any of the four others. In the latter case, two incorrect matches would automatically come about. If the first two adolescent girls' sketches were incorrectly matched, there would automatically be at least three, and quite possibly four errors.

TABLE I

PROBABILITIES OF THE CHANCE SUCCESS OF ONE JUDGE IN MATCHING PERSONALITY SKETCHES

Number of Successes	*Probable Chances in 100*	
	5 vs. *6 cases*	*10* vs. *13 cases*
0	43	46
1	37	36
2	15	14
3	4	3
4	0.07	0.06
5	0.01	0.008

Chapman (4, 5) has given formulae for the solution of this rather complex problem which are based on the possible permutations of the cases. In matching five versus six sketches, a total of 720 permutations are possible, but only one of these arrangements allows for the correct matching of all sketches. When there are four correct matches, and one error, there are five possible permutations, that is the sixth or extra infant sketch might be matched with any one of the five adolescent sketches. The chances of making one error, then, would be $\frac{5}{720}$. Other probabilities can be worked out by Chapman's formula. Table I shows the probabilities of the chance success of one judge in matching 5 versus 6 and 10 versus 13 cases worked out by this formula. It must be emphasized that these figures apply only to the chances of successful matching by one judge. The highest number of successful matchings obtained by any judge for the 5 versus 6 cases was four, and for the 10 versus 13 cases, five.

RESULTS

The results of 10 judges in matching the 5 adolescent sketches and 6 infant sketches of girls are shown in Table II. The majority of the judges succeeded at the 4 per cent level of probability, three succeeded at the .07 per cent level, and one at the 15 per cent level.

TABLE II

Successes of Ten Judges in Matching Personality Sketches of Girls (5 vs. 6 cases)

Number of Correct Judgments	*Probability Level*	*Numbers of Judges*
0	43%	0
1	37	0
2	15	1
3	4	6
4	0.07	3
5	0.01	0
		10 Total

The mean number of successes for all of the judges is 3.2. The significance of this figure can be calculated by another special method. According to Chapman (4), the mean of random matchings is, by sampling theory, $\frac{t}{u}$, ($t =$ the smaller number of items to be matched; $u =$ the larger number of items to be matched.) This agrees with Zubin's (28) discussion of the problem of matching equal numbers of items, in which the mean of random matchings is one, and the standard deviation is also one. For the more general case, which can apply with either equal or unequal numbers, Chapman gives the formula for the standard deviation. He gives also a formula for calculating the skewness of the distribution of random matchings which becomes more leptokurtic and more skewed as the difference between the numbers to be matched increases. The distribution takes the shape of Pearson's Type III Curve. Tables, which are correct to six places, have been worked out by Salvosa (21) for this function. Area under the Type III curve above the obtained mean value in terms of the standard deviation can be determined from these tables. Using this procedure, it was found that there is less than one chance in 1,000,000 that the mean of 10 judges in matching 5 versus 6 cases would equal or exceed 3.2.

The results of 5 judges in matching the 10 adolescent sketches of boys with 13 infant sketches, which are less clear cut, are shown in Table III. These results are less clear cut probably because of the difficulty of the task, and the difficulty of obtaining judges for the task. One judge did succeed, however, at the .008 per cent level. The mean number of successes for all of the judges in this case is 2.6. Applying Chapman's formula to this figure, there are only 25 chances in 100,000 that the mean of five judges in matching 10 versus 13 cases would equal or exceed 2.6.

TABLE III

SUCCESSES OF FIVE JUDGES IN MATCHING PERSONALITY SKETCHES OF BOYS (10 vs. 13 cases)

Number of Correct Judgments	*Probability Level*	*Numbers of Judges*
0	46%	0
1	36	1
2	14	2
3	3	1
4	0.06	0
5	0.008	1
		5 Total

The successes of individual judges as well as the high mean numbers of successes for both tasks as compared to chance demonstrate some common element in the sketches at both levels. Since the writer was not familiar with the earlier sketches nor with the pseudonyms used for the children at the time of preparing the later sketches, and since similarity existed in the sketches of the two-year-olds which were based on subjective thorough acquaintance, and the sketches of the same children at seventeen which were based on objective tests and rating material, we conclude that personality similarities exist.

Taking the subject rather than the judge as the unit may shed more light on the results. Table IV shows that one of the girls, Winnie, was always matched correctly by the 10 judges, while Judy was never matched correctly. It seems possible, in other words, to rank the subjects according to ease of matching: Winnie, whom 10 out of 10 judges matched correctly; Virginia Ruth, matched correctly by 9 judges; Sibyl by 7; Patty by 6 and Judy who was never matched correctly. In considering the problem of the probability of repeated successes in matching the same adolescent sketch, the only method of attack seems to be in considering the matching of each adolescent sketch as a separate task independent of other matching. This, of course, is not the case. As stated above, the chances of matching the first case are one in six, while after one sketch is correctly matched, the probability that a second will be correctly matched is one in five and so on. Considering each matching as a simple choice of one of six sketches also ignores the fact that the correct match for any given sketch may already be incorrectly matched elsewhere. For calculating the probability of repeated successes, nevertheless, the simplest method is one in which each adolescent sketch is considered separately, or as if it were the first to be matched. The probability that one sketch would be matched correctly by all judges, using this line of reasoning, is $\frac{1}{6_n}$ while other probabilities are worked out through use of the binominal expansion. This trend of difference between subjects in matchability was less definite for the boys on account of the small number of judges used. Fred and Harvey seemed to be most frequently matched, however. It is possible to interpret the trend in three ways. Ease of matching could be attributable, firstly, to the greater degree of similarity in personality in some cases. Perhaps, secondly, it might be due to more adequate description of some cases, at one or both levels. Allied with this is the third possibility that some individuals are more outstanding, and therefore can be matched more successfully than the generality even though the degree of similarity or dissimilarity between early and later status is similar for all.

TABLE IV

DISTRIBUTION OF TEN MATCHINGS OF INFANT AND ADOLESCENT PERSONALITY SKETCHES OF GIRLS

Adolescent Sketch	*Times Out of 10 Matched with Following Infant Sketch*					
	Winnie	*Va. Ruth*	*Sibyl*	*Judy*	*Patty*	*Carol*
Winnie	10					
Va. Ruth		9				1
Sibyl			7	3		
Judy			2		3	5
Patty				3	6	

TABLE V

PROBABILITY OF REPEATED SUCCESSES IN TEN MATCHINGS OF PERSONALITY SKETCHES

Adolescent Sketch	*Number of Correct Matches*	*Probability Level*
Winnie	10	0.00000165%
Virginia Ruth	9	0.0000828
Sibyl	7	0.00217
Judy	0	0.1615
Patty	6	0.000248

SUMMARY

Personality sketches of 19 children written by Shirley (24) on the basis of observations during the first two years of life were matched with personality sketches of 15 of the same children prepared by the writer on the basis of test and rating material. Later sketches were prepared without acquaintance with the earlier ones. Five judges matched 10 sketches of adolescent boys, and 10 judges matched 5 sketches of adolescent girls. Both the results of the individual judges and the mean scores of all judges in matching were significant as compared to chance. Following are the conclusions of the investigation.

1. Personality similarities in an individual persist over a period of time.
2. Some individuals are more readily indentifiable after a period of time, presumably due to greater uniqueness of personality pattern.
3. The matching technique, utilizing total impression, allows for the demonstration of similarities in personality pattern in the same individual over a period of time.

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Five—Two Instances of the Childhood Origin of Phobias

ENGLISH BAGBY

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These two cases indicate that severe maladjustments can arise from single traumatic experiences in childhood. Such case studies, however, cannot tell us what proportion of children, faced with the same situations, would have reacted so severely.

It is noteworthy that in each of these cases the forgotten episode that caused the phobia was recalled through the intervention of a person who participated in the event. Apparently recall of the repressed memory and subsequent readjustment took place without the assistance of a psychiatrist or psychologist.

CASE I

A young woman of good heredity developed during her childhood a severe phobia of running water. She was unable to give any explanation of her disorder, which persisted without noticeable improvement from approximately her seventh to her twentieth year. The general nature of the disturbance is readily shown.

Her fear reaction to splashing sounds was especially intense. For instance, it was necessary for her to be in a distant part of the house when the bathtub was being filled for her bath and, during the early years, it often required three members of the family to give the bath. She always struggled violently and screamed. During one school session a drinking fountain stood in the hall outside of her classroom. If the sound of children drinking was audible, she became very frightened, actually fainting on one occasion. When she rode on railroad trains, it was necessary to keep the window curtain down so that the streams over which the train passed might not be seen.

During her twentieth year an aunt, Mrs. G., came to visit at her home. This lady had not seen her niece in 13 years. She was met at the station by the mother of the girl, who told her of the daughter's condition. On arrival at the house, she met the girl and her first words were "I have never told." This served to provoke a recall of the following episode.

The mother, the aunt, and the little girl—she was then seven years old—had gone on a picnic. Late in the afternoon the mother decided to return home but the child insisted that she be permitted to stay longer with her aunt. The mother agreed to this on the child's promise to be obedient to the aunt. The two then went into the woods for a walk and the girl, disobeying her aunt's instructions, ran off alone. The aunt followed, and after a search, found the child lying wedged among the rocks of a small stream with a water-fall pouring down over her head. She was screaming with terror. They proceeded to a farm house where the wet clothes were dried. The child expressed great fear that her mother would learn of her disobedience, but the aunt reassured her with the promise, "I will never tell." They returned home and the aunt left the house next morning without seeing her niece. The child was thus left with no one in whom she could confide and had a period of anxiousness. The phobia developed shortly after this.

After recalling this experience of her childhood, the young woman found it possible to approach running water without discomfort. And gradually the special adjustments, which her phobia had necessitated, disappeared.

CASE II

A man suffered from a phobia of being grasped from behind, the disturbance appearing early in childhood and persisting to his fifty-fifth year. When walking on the street he was under a compulsion to look back over his shoulder at intervals to see if he was closely followed. In social gatherings he arranged to have his chair against the wall. It was impossible for him to enter crowded places or to attend the theater.

In his fifty-fifth year he returned to the town in which he had spent his childhood. After inspecting his old home, he went to the corner grocery and found that his old boyhood friend was still behind the counter. He introduced himself and they began to reminisce. Finally the grocerman said this, "I want to tell you something that occurred when you were a boy. You used to go by this store on errands, and when you passed you often took a handful of peanuts from the stand in front. One day I saw you coming and hid behind a barrel. Just as you put your hand in the pile of peanuts, I jumped out and grabbed you from behind. You screamed and fell fainting on the sidewalk."

The episode was remembered and the phobia, after a period of readjustment, disappeared.

The following features common to the two cases seem significant. In fact, the claustrophobia described by Rivers in his *Instinct and the Unconscious* and Prince's "Bell-tower Case" are capable of the same analysis.

1. The disturbance dates from a traumatic episode to which the patient reacted with intense fear.
2. The episode involves some forbidden action on the part of the patient which prevents discussion of the experience with parents or others who might give reassurance. This element of guilt and the unpleasant character of the experience presumably cause "protective forgetting," or repression.
3. The symptoms disappear when recall is effected. No sublimation, transference, or treatment by suggestion was required.

Freud has formulated an explanation of phobias. His view is set forth in *A General Introduction to Psychoanalysis.* He finds that phobias arise in individuals who have permitted their libido, or sex-energy, to find outlet in some forbidden practice. With the suppression of this practice through social influence, the sex-energy is transformed into free-floating fear. Later this fear becomes attached to certain features of the environment. It is suggested that the stimulus to which the phobia attaches may be subtly related to the original sex-interest of the individual. Thus perhaps, a fear of running water represents an underlying urinary eroticism.

It seems clear that Freud's view is not required for explanation or treatment of the two cases presented. Neither seems to involve sex factors. However, it is easy to see how a sex act would, in many cases, supply the element of guilt which is found in the traumatic episode.

Six—Personality Organization in Children

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There are many divergent views concerning the role of childhood experiences in the formation of personality. Anderson, for many years director of the Institute of Child Welfare at the University of Minnesota, presents an over-all evaluation of this problem.

It is my purpose to examine some of the current conceptions of the personality of young children in the light of modern research, and more particularly, the implications of erroneous conceptions that have far-reaching effects in determining policy and practice with children.

Many discussions of child personality found in textbooks in general, abnormal, and social psychology are written from an adult point of view by persons who have had little contact with children. Many of the statements made seem to be projections backward upon the child of concepts that seem necessary for the author's conception of adult behavior. In the main, the impression given is that the young child is passive, delicate, unusually subject to shock and trauma, and responsive to all possible stimuli. For instance, some advocates of the frustration-aggression hypothesis picture the child as a passive person who reacts when he is blocked and who expends energy in proportion to the amount of frustration. So far so good. But when it is assumed that all aggressive activity in young children is the outcome of frustration, the active display of energy which is the most fundamental characteristic of a living organism activity is denied, and docility and passivity are attributed to the young child as normal.

This is also the purport of much of the psychoanalytic teaching about children. The adult is what he is, not because of inadequacy in his makeup or training, but because of a traumatic experience. Since this experience cannot be found in the present, it is pushed back into an earlier period. When it cannot be found there, it is pushed still further back. Thus, the trauma of adolescence moves back into late childhood, then into early childhood, then into infancy, then to birth, and then into the fetal period. The only place left for trauma is conception.

The analytic and case history writings abound with instances of traumatic and dramatic episodes that are presumed to affect later development. But if the data do not support this view, there is an easy out since such episodes can be repressed and still function if it is assumed that the child remembers everything. Thus, Symonds (28) in citing the Landis study (17) reports correctly when he says: "Little children who are the recipients of sexual advances from adults usually have no memory of them," and quotes Landis in a footnote to the effect that only 3 out of 295 individuals reported such incidents as occurring before five years. But Symonds (28) goes on to say:

> They become deeply repressed. However they may later cause personality disturbances and grievous conflicts which are difficult to manage. Likewise almost any traumatic or painful experience of childhood is rapidly forgotten, which means it is repressed. Children seldom remember the extreme dangers encountered in early childhood, the extreme punishments which they received, the accidents which befell them. Where such memories persist these are not usually the really dangerous, harmful, painful experiences; it has been found that they represent screen memories which protect one from the memories of really traumatic events."

Recently Ribble (22) in her book on *The Rights of Infants* has emphasized the infant's need for security and affection and traces almost every type of mental and physical disturbance in infancy and early childhood back to lack of affection. So extreme are her statements that almost every review in scientific journals has pointed out many misinterpretations of established fact. For the understanding of children, Ribble would substitute love and affection. Such conclusions make little difference if read only by scientific workers competent to judge the evidence for themselves; unfortunately, however, the book has become a Gospel among social workers who in one instance, took the children out of home management houses by citing the Ribble generalizations, even though all the objective evidence obtained by medical examinations and the case histories of the former children showed excellent health and good adjustment as did also the scientific studies published on this problem. Policies of many years' standing with demonstrated successful outcomes in terms of child behavior went by the board almost without protest. Not long ago a social agency discussed seriously whether or not infants should be kept temporarily in an institution for several months for examination and study prior to adoption, on the ground that life in an institution for one or two months would make the infant insecure and cause later personality difficulties. No one seemed aware of the fact that in ordinary homes children are sometimes away from loved ones for months without serious consequences and that adoption itself forces new emotional attachments which are readily made by infants.

In the otherwise excellent study of children's paintings by Alschuler and Hattwick (2), an example of another error is found. In the first volume, many adult characteristics are read into children's paintings by making symbolic interpretations in terms of adult values. Thus, it is said that the child's use of browns and reds and yellows which are "smeary" indicate his concern with eliminative processes, and a little later on a somewhat similar statement is made about the child's use of green. When the statistical material in the second volume is examined for validation of these generalizations, no attempt to check the use of various colors against the children's eliminative habits is reported.

At a recent panel discussion another error was made. A participant talking on jealousy described the situation of the jealous child as like that in which a husband tells his wife that he is bringing a new wife into the home, expatiates on her desirability and asks his wife to assist in preparing for the new wife's coming. While this analogy has obvious dramatic qualities, it is far from a good description of child behavior. There is little evidence that marked jealousy on the arrival of a new member of the family is frequent. However, the evidence available has been played up in the practical writings and jealousy quite appropriately ascribed to an unwise distribution of affection.

But a young child is not like an old wife who has a whole series of attitudes reinforced by memory and by experiences tied in with the moral, social, and institutional systems of a monogamous society. Nor are the neighbors' valuations of conduct—so essential in the situation faced by the wife—present in the young child who reacts more specifically to the situation and less to the complex of background factors. Which, then, is the more tender, the wife who has been socially sensitized or the young child who has not?

Various generalizations on the personality of the infant and young child which follow from these impressions and interpretations can be made: (a) Infants and young children are essentially passive recipients of stimulation who display little energy or activity on their own; (b) are very delicate and tender, and have little capacity to resist or survive and are especially sensitive to lack of affection to which they cannot adapt for even short periods; (c) are unusually susceptible to, and carry the effects of, traumatic episodes indefinitely; (d) carry forward all their memories and experiences, which later come out to plague them; (e) in their behavior and the products resulting, are subject to all the implications and values that inhere in adult reactions in similar situations.

In contrast to these views of child nature, scientific findings which lead to a more accurate picture may be considered. These can be grouped under several heads: the child as an energy system with high capacity for self repair and selectivity; the child as a persistent personality system with high capacity to resist deformation, stress, and trauma; and the child as a mechanism that meets life not as a storehouse or filing cabinet, but as an active system engaged in transforming input into outgo.

In living animals with their capacity to move about, life and activity are closely related phenomena. The inactive organism is dead. A live animal is an active center for transforming food into energy and stimulation into activity. The young organism is more active than the older one, and expends energy at a higher rate. Given normal conditions of health, high motility and activity characterize the waking hours. To produce inactivity, it is only necessary to cut down the food intake for a period long enough to outlast the first increase in motivation. When intake is thus cut, children adjust by becoming lethargic and inactive. When normal food levels are restored, energy expenditure increases.

Insight into the child's effectiveness as an energy system can be gained from a study of the energy maintenance process in both animals and humans. Brody (6) in his recent book on *Bio-Energetics* has brought much of this material together. With increase in size, maintenance cost in terms of energy becomes greater, because more effort is necessary to push the weight around. In younger children with low body weight and slight mass in comparison with the mechanism available for motion, energy can be expended at a very high rate at a low cost to the organism. Thus, as size increases, more energy is needed for maintenance and less is available for both growth and work. While no studies over a long span of years under comparable conditions have been made of the activity level of humans, the animal studies indicate that with growth and increase in body size, total activity lessens. Casual observations together with informal checks covering all activities in the attempt to measure total energy output make it seem clear that as an energy expending mechanism the infant and young child are very efficient. But if measurement is made in terms of direction of output, that is, effectiveness in accomplishing a particular piece of work, the results are not so favorable to the child. The child's energy expenditure is essentially unstructured, whereas that of the older workers is highly disciplined. In fact, skill consists largely in doing more with less effort. Thus in terms of the energy available, the young child is not tender or delicate. He is so lively and active that he tends to wear out the inferior adult. As adults we look upon growth as gain. But in terms of total energy growth is loss. Efficiency seems to be gained at the cost of energy.

As an energy system the child is composed of many interrelated parts which work together as a functional unit. Within the system as a whole, and even within the parts of which it is composed, there is much opportunity for compensation and adjustment in order to meet external stress and strain. This principle can be readily demonstrated for the body as a whole and for any of the great organ systems as well; it is one of the basic devices by means of which life is maintained in the face of an environment.

Not only is there capacity for compensation and adjustment, but also tremendous capacity for self-repair when damage occurs. Within limits, this is also a property of all living systems. Evidence indicates that this capacity for self-repair decreases with age. For example, in the deNouy (21) investigations of the rate at which cuts heal, it was found that in the ten-year-old child cuts heal twice as fast as in the twenty-year-old, in whom they heal three times as fast as in the sixty-year-old. Thus, in the ten-year-old child the healing rate is six times that of the sixty-year-old. If these results can be generalized to other processes and extrapolated, it would appear that a physical shock or trauma to produce the same amount of deformation in a ten-year-old would have to be twice as severe as that for the adult, and six times that for the sixty-year-old.

As evidence against this view, the great sensitivity of the young child to drugs and toxic substances of various types might be cited. This, however, seems to be much more a function of the mass of the body than of its reactiveness as is shown by its variation with differences in weight in species and individuals and in the use of simple linear conversion formulae. Infants and young children, however, show very quick recovery rates from the drugs given in doses appropriate to their weight. Similarly, they recover quickly from toxic effects and from illness.

Students of the physical effects of birth, while emphasizing the rigor of the birth process for the child, also stress the tremendous capacity of the infant to withstand the shock. It has been said that for the normal infant, the physical shock of being born is about the equivalent to an adult being thrown out of a third-story window. Yet normal infants take this shock in their stride, without serious aftereffects or bodily difficulties.

Being prematurely born should constitute a trauma of the first magnitude, not perhaps physically, but certainly psychologically if generalizations of the type made by Ribble and the psychoanalysts are correct, because of the forced separation of the infant from its mother well in advance of the appropriate time. Yet a substantial amount of writings in anatomy, physiology, and psychology shows that prematures cannot be distinguished by any criteria whatever from full-term infants after nine months in most cases, and after eighteen months in all cases.

Another characteristic of a living system is the selective character of its reactions to the environment, that is, the manner in which it preserves its essential unity by choosing that which fits the system, and neglecting or rejecting that which is not congruent. This is also a protective device that enables the child to insulate himself against unwanted stimulation and to resist shock and trauma.

Are the infant and young child more or less delicate than older persons? If this problem, thus far studied to only a limited degree, were analyzed in terms of healing times, repair rates, recovery from illness, and the rapidity with which lost function is regained, in my opinion the results would be very favorable to the infant and young child, not quite so favorable to the adult, and much less so to the old person. Children have an amazing capacity to survive in the face of the various demands made upon them. By and large, most life processes look toward the preservation of the young in preference to the older organisms.

But there is also a more direct answer to this question. What do the scientific studies, carried on over a span of years, reveal with respect to the persistence or nonpersistence of personality traits? If the personality of the infant and young child is so delicate, it should be modified easily and show great change from year to year and over a period of years; if it is a system that resists deformation and maintains its integrity, it should show less change with time, and the changes, when and if they occur, should be consistent with the original personality.

With the cumulation of data on children as a result of many years' observation in modern centers, this problem is being attacked for a wide variety of traits and trait patterns, in addition to the many studies of the persistence of intelligence which on the whole support the view of stability.

Baby biographers of the older days (12, 25) noted the emergence of individuality in infancy and emphasized the persistence of temperamental or personality traits. Woolley (30) described Agnes, a little girl who showed the same personality pattern of aggressive behavior from ages two to five. Studies on groups of children are found in the more recent writings. Cushing (8) found persistence in perseveration, Washburn (29) in smiling, Brackett (5) in laughter, and Bayley (4) in crying. Green (11) found persistence for group play and Loomis (18) for social contacts. Jersild and Markey (13, 15) found conflict behavior similar over a year's time. Stutsman (27) studying 140 preschool children for 3 consecutive years found high consistency in ratings on traits and in profile patterns. In the McGraw study (19), Johnny and Jimmy maintained personality differences as long as they were studied. Allport (1), studying his own son, found that the prognosis of personality development at four months of age tended to be borne out at two later ages. Gesell (9) showed movies of five children at ages one and five to trained observers who ranked the children on 15 behavior traits, including energy, demeanor, ascendance, social responsiveness and the like. Out of 75 rankings, 48 coincided, 21 were displaced by only one rank, 5 by two ranks, and 1 by three ranks. He concludes that this shows the prophetic character of the first year's behavior traits.

In the study by Sanford, and others (24), a restricted sample of children was studied over a three-year period on an unusually wide variety of personality variables. To quote:

> The results analyzed indicate that, in general, subjects do tend to be consistent in their behavioral manifestations over the three-year period to a degree that approaches statistical significance, (average r equals .33). Subjects who rate high as compared to their fellows, on one of the variables present in 1937 also rate high in 1938 and in 1939. On the other hand, the present coefficients of consistency are not large, and it is clear that subjects vary considerably, in standing on the different variables, from one year to another. How much of this variation is due to the fact that subjects actually change and how much is due to the unreliability of the measures cannot be determined from our data. (p. 236)

Roberts and Fleming (23) analyzed the persistence and change in personality patterns in the precollege, college, and postcollege periods for 25 women whose life histories were obtained through four years of contact with a college advisory service. In each case, some nucleus of traits persisted throughout the whole period of observation. In each there was also some fluctuation in traits. Although the whole persistence was greater than fluctuation, the amount varied from case to case. At one extreme, one woman showed 85 per cent persistent traits, whereas at the other, one woman had only 26 per cent persistent traits. For the group as a whole the percentage of persisting and fluctuating traits was in the ratio of 3:2. Of the 25 women, 16 showed more, 6 showed an equal amount, and 3 showed less persistence than fluctuation.

Kinder (16), has reported a study of 15 infant chimpanzees ranging in age from one year four months to four years three months, each studied five times over three and a half years. She says: "Characteristics appearing at the earliest ages studied have remained relatively constant over the three and a half years as shown by high statistical retest reliability." There was also evidence of highly specific and constant social relationship for certain pairs.

The most extensive study of persistence over a long span is that of Neilon, who followed up the infants for whom Shirley (26) had written detailed personality sketches based on her extensive studies of them from birth to two years. Neilon, who had not seen these sketches, studied as many of these children as could be located 16 years later with a variety of personality tests, and a standardized interview on attitudes, interests, and activities. In addition, she interviewed each mother, and had both mothers and fathers rate the children on a wide variety of traits. From this data a lengthy personality sketch was written for each at the age of seventeen years.

The original Shirley sketches were then carefully checked against the Neilon sketches to eliminate any obvious cues or overlapping factual statements. Ten judges then matched 5 sketches of adolescent girls with the sketches of 6 infant girls, among which were the same 5. Five judges matched 10 sketches of adolescent boys with 13 infant sketches which included the same 10. For the girls there was less than one chance in a million that the mean of the 10 judges in matching 5 versus 6 cases would equal or exceed 3.2. For the boys there were only 25 chances in 100,000 that the mean of 5 judges in matching 10 versus 13 cases would equal or exceed 2.6. By any statistical method the matching was very far above chance. Analysis showed much greater ease of matching for some children than for others. Thus, it becomes clear that personality characteristics persist in an individual over a period of time, in this instance at least 16 years from infancy to adolescence, and that some personalities are more readily identifiable after a period of time, presumably because of a greater uniqueness of personality pattern than are others. As with the earlier studies, this study supports the principle of the consistency rather than the inconsistency of personality traits.

It is one of the paradoxes of our modern discussions of personality that those who are most insistent upon the responsiveness of the personality to shock and trauma are also those who believe most strongly that the individual is a vast storehouse of persistent memories which underlie his subsequent behavior. How the contradiction between changing personality and permanent memories is to be resolved is not clear. Let us turn to the material on memory and learning in young children.

The studies of learning and memory in young children show results that are similar to those obtained on older persons (3), namely, a rapid forgetting rate unless there is restimulation and overlearning. Young children show tremendous repetitive activity and much overlearning rather than marked superiority in retention itself. Some superiority in young children might be expected from priority in learning and lack of interference. But results show that memory for both nonsense and meaningful material increases with age.

Pertinent to the problem of retention is the amount of experience young children have. Although most adults assume relatively slight amounts of stimulation and response, the evidence obtained in studies of young children is quite the reverse. The mass of impressions crowding in, and the number of specific responses made within a single day, and the number of potential memories created even in a short period is tremendous. For instance, studies on language development show that four-year-olds speak as many as 10,000 words per day, which multiplied by 365 for the year begins to reach astronomical figures. Studies of social contacts of young children reveal hundreds of teacher-child, and child-child contacts within a relatively short period of time. In a reasonably stimulating environment, the sheer quantity of interrelations tend to be overpowering and raises a question how out of such a mass of materials, specific items can be retained.

Not often considered in connection with child memories and traumatic episodes is the information on the learning of skills, in which control groups and checks for retention have been used. On the whole, most of these studies have yielded negative results for continuation of skill at the terminal practice level. Even within a short time there is substantial loss. These results have been somewhat discouraging, particularly to those who expect to train and find an intact result years later. Unless there is occasional practice to keep a particular skill or activity alive by occasional practice, it tends to regress or drop to a low level. In those instances, such as the Jersild and Bienstock (14) experiment on the persistence of musical skill after an interval of two years, a theory in terms of sensitization and implicit practice as a result of musical stimulation in the environment is developed to account for the differences in the two groups. In actual life in contrast to experimental situations, the child who learns to swim early, tends to swim much more than the child who learns late and thus maintains superiority. The learning material not only indicates the child's enormous capacity to forget, but also shows that much forgetting is quite complete.

Additional light is thrown on the problem of the persistence of personality traits by a whole group of studies not often considered in connection with the problem of personality organization. These also show failure to retain new modes of behavior and adjustment in which training has been given. I refer to the follow-up studies of delinquent behavior, of which the most striking example is that of Glueck and Glueck (10), who found extraordinarily high proportions of recidivists when every case that had been through guidance clinics and juvenile court treatment was followed up. This discouraging result, however, should only give us pause; on the whole, after their curative and remedial treatment, these children had to return to their old environments in which all the factors which produced the original delinquency were still operative.

While many stimulating and brilliant researches have shown the possibilities of modifying social behavior by measuring the child's responsiveness to his group, giving intensive training apart from it, and then measuring his responsiveness on his return, few checks for retention are seldom made. Hence, no one knows how long, subsequent to training such modifications will persist without reinforcement. If the personality is a selective system operating on its environment in accordance with a principle of congruence, care must be taken in interpreting the effects of such experimental or training procedures.

The implications of the foregoing view are reasonably clear. Instead of being a passive and receptive organism, the young child is a personality system that functions as a unit in its present environment. Available in the present is an energy mechanism which, because of some degree of organization, is selective in relation to its environment. Of the countless experiences in an almost continuous round of stimulation and response, some are retained and incorporated into the system, while very many are lost and lost permanently.

How, then, are the temporary experiences distinguished from those which are transformed from the situation into the internal system of the individual? How is the developmental stream of personality manifestation modified? For temporary experiences, the response is related quite directly to the stimulus and is determined by the valences within the situation. With more permanent modifications, the effects produced within the organism or by the organism on the external environment may have little or no relation whatever to the intensity of stimulus or the immediate valences which function merely as cues to set off a response which thenceforth goes on its own course. Thus, the personality achieves a type of inner organization which becomes more and more powerful with the passage of time, and which frees the personality to a greater and greater degree from the dominance of the situation.

If this principle holds, a traumatic experience is not really an episode but an event which modifies the stream of organism-environment relations. The factors which make an experience traumatic for one child and not for another are precisely those which determine the permanence or impermanence of memories. In order to modify the stream of organism-environment relations there must be reiteration, that is, a mechanism similar to that which produces overlearning in the case of a skill or memory must be present. It then becomes important to examine the environment for recurring stimulation and the individual for manifestations of an internal process that can exercise a "carrier" function, which operating again and again, will keep alive the subsystems of response which are crucial in personality organization. Such an internal mechanism is found in speech and the symbolic processes which are universally used by those responsible for children to control and educate them and by the person himself either overtly or implicitly to fix and control his own behavior. While some research has been done on these controlling functions, when used on others little has been done on the devices by means of which the individual controls himself.

In my opinion, promising opportunities for future research lie in the analysis of the organism-environment relation in terms of the repetitive and reiterative phenomena occurring within the constant stream of stimulation and response that serve to keep skills, experiences, and memories alive and which make of certain experiences, traumatic episodes. The following suggestions as to possible devices may be made.

1. *The family story.* Many of the young child's experiences occur under conditions in which they are observed by others and become the occasion of family stories. Thus, in one family one child behaved bravely and another in cowardly fashion in the face of a

fearsome situation. The story of Mary's fear and Johnny's bravery was told again and again in their presence; in fact, the children came in time to ask that the story be told. Here is not only a reinforcing mechanism but also one that sets up an expectancy with regard to future behavior.

2. *Consistent presentation of attitudes and roles.* In the recent study by Child, and others (7) of children's textbooks and personality development, an environmental factor that involves reiteration was located. By analyzing a large number of school readers available to children, it was found that characteristic attitudes were consistently presented. Thus, the school readers regularly presented the boy as active, seeking adventure, and achieving, and the girls as sociable, kind, timid, and relatively uncreative. Although no similar analysis of consistent emphasis and values which parents and associates put upon certain types of stimulation and behavior has been made, it seems clear that such analysis would reveal similar factors operating.

3. *Attention-getting devices.* Another type of reiteration may be found in the child's accounts of his own experiences in order to secure attention. In a brief moment of high excitement all others turned to him. Why not recapture it by retelling? If the audience responds, all well and good, if not, the child blocked from overt expression can turn to silent speech and fantasy. Here again will come reinforcement and expectancy.

4. *Return again and again to experiences because of their incompleteness.* Actions cutting the child's experience short may bring an experience out of its background and cause its repetitive behavior. The mother's unsatisfying answer to a question about sex in comparison to her direct answer to other questions may cause the child to return again and again to the question. A child told not to touch a gas stove could hardly keep away from it, while another shown how to operate the valves, quickly lost interest.

5. *Persistent problem of adjustment.* In the Alschuler and Hattwick study of *Painting and Personality*, Aileen who had a continuing adjustment problem, made paintings for many years with a common element, which apparently had a symbolic relation to the problem.

6. *Group expectancies and roles.* Within a group of children, roles are assumed by individuals who thereafter are expected by the group to take these roles on subsequent occasions. In time persistent patterns of behavior in terms of group expectancies emerge. Here are reiterative relations which force conformity because of their recurrence and which mold personality.

SUMMARY

The personality of young children is frequently misrepresented because of views based on adult behavior which project back upon the child adult characteristics. Common types of misrepresentation are: (a) the child is passive and responds to all the stimulation to which he is exposed without action or selection on his part: (b) the child is so delicate and tender that he must be protected at all costs, and must have exceptional amounts of love, affection, and security; (c) the child carries forward all his memories and experiences to later behavior, and is particularly affected by so-called "traumatic" episodes or single intense experiences.

In contrast to these, the following picture of the child as a personality system is presented: (a) The child is an active energy system that responds selectively to its environment and neglects as well as takes in many stimuli; (b) the child has substantial capacity to withstand stress and strain, and recovers quickly from deformation and has great capacity for self-repair and readjustment; (c) the child lives in the moment rather than in the past or future. What affects him must somehow or other get into the field of forces in which he is reacting; (d) the child forgets much of what he experiences and carries forward as mental and behavior equipment only that part of his experience which, because of reiteration has received unusual emphasis; and (e) because the stream of stimulation and reaction in the normal young child is tremendous, the quantity of experience had within even a short time far exceeds that which persists.

Personality becomes organized around certain nodal points or experiences which have received particular emphasis and much reiteration. As examples of reiterated factors that give particular experiences significance were mentioned: (a) the oft-repeated family story which fixes behavior and creates expectancies; (b) repeated environmental simulation consistent in attitude and role; (c) accounts of experiences which because they attract the attention of others are repeated either aloud or silently; (d) the return again and again to incomplete and unsatisfactory experiences; (e) persistent problems of adjustment which force symbolization; (f) group expectancies which create roles that are assumed again and again.

The child, then, is not a simple passive creature molded exclusively by external forces; he is very much a creature in his own right, moving through his own experiences and creating his own world. This is not to deny the value of nurture in creating the best possible world for children, but to plead for a study of the child as he is rather than to use him as a convenient locus for images which fit adult conceptions and convenience.

What I have said might be interpreted as indicating that personality traits are unmodifiable, and to deny the value of good nurture. Such is not my view at all. What we need, it seems to me, is a much more complete approach to the problem of persistent and fluctuating traits and characteristics with an analysis of all our data in terms of a span of time. When this is done we will locate the factors that produce modifications and find that change is orderly and consistent. We will move away from emphasis upon the traumatic episode, which is but another instance of what E. B. Holt called the "great white flame" theory of behavior. By locating in the stream of stimulation and response the recurring factors which are subject to control, we will be able to guide the process of growth and development more effectively and improve the adjustment of all children. We not only need to know how *to create* a good environment for children; once created, we need to know how *to maintain it.*

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IX–SOCIAL FACTORS IN CHILD BEHAVIOR

One—The Form of the Family and Child Behavior

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Child psychology developed in Europe and America. Malinowski, an anthropologist, warns that family structure differs markedly in different societies, and that the attachments, taboos, and pressures of Western civilization are not universal. He himself spent much time among the Trobriand Islanders of Northeastern New Guinea. The following is one of his early accounts of the Trobriand Island family and the child's place in it. Fuller accounts are presented in his SEX AND REPRESSION IN SAVAGE SOCIETY, THE FATHER IN PRIMITIVE SOCIETY, *and* THE SEXUAL LIFE OF SAVAGES.

THE CONSTITUTION OF THE FAMILY IN A PATRIARCHAL AND IN A MATRILINEAL SOCIETY

I do not propose, of course, to survey all forms of human family, but shall compare in detail two types, known to me from personal observations: the patriarchal family of modern civilization, and the matrilineal family of certain island communities in Northwestern Melanesia. These two cases, however, represent perhaps the two most radically different types of family known to empirical observation, and will thus serve our purpose well. A few words will be necessary to introduce the Trobriand Islanders of Northeastern New Guinea (or Northwestern Melanesia) who will form the other term of our comparison.

These natives are matrilineal, that is, they live in a social order in which kinship is reckoned through the mother only, and succession and inheritance descend in the mother line. This means that the boy or girl belongs to the mother's family, clan, and community; the boy succeeds to the dignities and social position of the mother's brother, and it is not from the father but from the maternal uncle, or maternal aunt, respectively that a child inherits its possessions.

Every man and woman in the Trobriands settles down eventually to matrimony, after a period of sexual play in childhood, followed by a general license in adolescence, and later by a time when the lovers live together in a more permanent intrigue, sharing with two or three other couples a communal "bachelor's house." Matrimony which is usually monogamous, except with chiefs who have several wives, is a permanent union, involving sexual exclusiveness, a common economic existance, and an independent household, and outwardly at first glance it might to a superficial observer appear the exact pattern of marriage among ourselves. It is, in reality, entirely different. To begin with, the husband is not regarded as the father of the children in the sense in which we use this word; psychologically he has nothing to do with their birth, according to the ideas of the natives, who are completely ignorant of physical fatherhood. Children, in native belief, are inserted into the mother's womb as tiny spirits, generally by the agency of the spirit of a deceased kinswoman of the mother. Her husband has then to protect and cherish the children, to "receive them in his arms" when they are born, but they are not "his" in the sense that he has had a share in their procreation.

The father is thus a beloved, benevolent friend, but not any recognized kinsman of the children in any sense. He is a stranger, having authority through his personal relations to the child, but not through his sociological position in the family. Real kinship, that is, identity of substance, "same body," exists only through the mother. The authority over the children is vested in the mother's brother. Now this person, owing to the strict taboo which prevents all friendly relations between brothers and sisters, has never been and can never be an intimate of hers or therefore of her household. She recognizes his authority, and bends before him as a commoner before a chief, but there can never be tender relations between them. Her children are, however, his direct heirs and successors, and he wields over them direct power. At his death his wordly goods pass into their keeping, and during his lifetime he has to hand over to them any special accomplishment he may possess—dances, songs, myths, magic, and crafts. He also it is who supplies his sister and her household with food, the greater part of his garden produce going to them. To the father, therefore, the children look only for loving care and tender companionship. Their mother's brother represents the principle of discipline, authority, and executive power within the family.

The bearing of the wife toward her husband is not at all servile. She has her own possessions and her own sphere of influence, private and public. It will never happen that the children will see their mother bullied by the father. On the other hand, the father is only very partially the breadwinner, and has to work mainly for his own sisters, while the boys know that when they grow up they in turn will have to work for their sister's households.

Marriage is patrilocal, that is, the girl goes to join her husband in his house and migrates to his community, if she comes from another, which is the general case. So that the children grow up in a community where they are legally strangers, having no right to the soil, no lawful pride in the village glory; while their home, their traditional center of local patriotism, their possessions, and their pride of ancestorship are in another place. Strange combinations and confusion arise associated with the dual influence to which I allude.

From an early age boys and girls of the same mother are separated in the family, owing to the strict taboo which enjoins that there shall be no intimate relations between them, and that above all any subject connected with sex should never interest them in common. It thus comes about that, though the brother is really the person in authority over the sister, the taboo forbids him to use this authority when it is a question of her marriage. The privilege of giving or withholding consent, therefore, is left to the parents, and the father—her mother's husband—is the person who has most authority, just in this matter of his daughter's marriage.

Thus the great divergency of the two families which we are going to compare is beginning to be clear. In the normal family with us we have the authoritative, powerful husband and father backed up by society. We have also the economic arrangement whereby he is the breadwinner, and can—nominally at least—withhold supplies or be generous with them at his will. In the Trobriands, on the other hand, we have the independent mother, her husband who has nothing to do with the procreation of the children, and is not the breadwinner, who cannot leave his possessions to the children, and has sociologically no established authority over them. The mother's relatives are endowed with very powerful influence, especially her brother, who is the authoritative person, the producer of supplies for the family, and whose possessions the sons will inherit at his death. Thus the pattern of social life and the constitution of the family are arranged on entirely different lines.

It might appear that while it would be interesting to survey the family life of the matrilineal society, it is superfluous to dwell on our family life, so intimately known to every one of us. We might simply take it for granted. But first of all, it is essential in a strict comparative treatment to keep terms of the comparison clearly before our eyes; and then, since the matrilineal data here to be given are collected by special methods of anthropological field work, it is indispensable to cast the European material into the same shape as if it had been observed by the same methods and looked at from the anthropological point of view. It is obvious that the infantile conflicts will not be the same in the lavish nursery of the wealthy bourgeois as in the cabin of a peasant, or in the one-room tenement of a poor working man. It would be important to consider not only the higher classes but also the lower and the ruder strata of society, where a spade is called a spade, where the child is in permanent contact with the parents, living and eating in the same room and sleeping in the same bed, where no parent substitute complicates the picture, no good manners modify the brutality of the impact, and where the jealousies and petty competitions of daily life clash in hardened though repressed hostility.

In order to make the comparison stand out clearly I shall divide the history of childhood into periods, and treat everyone of them separately, describing and comparing it in both societies. For the present purpose it will be enough to distinguish four periods in the development of the child, defined by biological and sociological criteria.

1. *Infancy*, in which the baby is dependent for its nourishment on the mother's breast, and for safety on the protection of the parent, in which he cannot move independently nor articulate his wishes and ideas. We shall reckon this period as ranging from birth to the time of weaning. Among primitive peoples, this period lasts from about two to three years. In civilized communities it is much shorter —generally about one year only. But it is better to take the natural landmarks to divide the stages of childhood. The child is at this time physiologically bound up with the family.

2. *Babyhood*, the time in which the offspring, while attached to the mother and unable to lead an independent existence, yet can move, talk, and freely play about around her. We shall reckon this period to take up three or four years and thus bring the child to the age of about six. This term of life covers the first gradual severing of the family bonds, The child learns to move away from the family and suffice unto himself.

3. *Childhood*, the attainment of relative independence, the epoch of roving about and playing in common with other children.

This is the time also when in all branches of humanity and in all classes the child begins in some way or other to become initiated into full membership of the community. Among some savages, the preliminary rites of initiation begin. Among others and among our own peasants (and working people) the child begins to be apprenticed to his future economic life. Members of civilized communities begin their schooling at that time. This is the period of the second severing from family influences, and it lasts till puberty, which forms its natural term.

4. *Adolescence,* between physiological puberty and social maturity. In many savage communities, this epoch is encompassed by the principal rites of initiation, and in other tribes it is the epoch in which tribal law and order lay their claim on the youth and on the maiden. In modern civilized communities it is the time of secondary and higher schooling, or else of the final apprenticeship to the life task. This is the period of the complete emancipation from the family atmosphere, and among savages and our own lower strata it normally ends with marriage and the foundation of a new family.

Let us briefly summarize and characterize the social codeterminants of motherhood in our own society. Maternity is a moral, religious, and even artistic ideal of civilization, and a pregnant woman is protected by law and custom, and should be regarded as a sacred object, and she herself ought to feel proud and happy in her condition. That this is an ideal which can be realized is vouched for by historical ethnographical data, and even in modern Europe, the othodox Jewish communities of Poland keep it up in practice, and amongst them a pregnant woman is an object of real veneration, and feels proud of it. In the Christian Aryan Society, however, pregnancy among the lower classes is made a burden, and regarded as a nuisance; among the well-to-do people it is a source of embarrassment, discomfort, and temporary ostracism from ordinary social life. Since psychoanalysis has recognized the importance of the mother's prenatal attitude for her future sentiment toward her offspring, and since this attitude varies greatly with the milieu and depends on social values, it is important that this sociological problem should be studied more closely.

At birth, the biological arrangements and the impulses of the mother are endorsed and strengthened by society, which in many of its customs, moral rules, and ideals, makes the mother the nurse of the child, and this broadly speaking, in the low and high strata of almost all nations of Europe. Yet even here in such a fundamental, so strongly biologically secured relation, there are certain societies where custom and laxity of true morals allow of notable aberrations.

Thus we have the barbarous system of sending the child away for the first year of its life or so to a hired foster-mother, once highly prevalent in the middle classes of France, or the almost equally barbarous system of protecting the mother's bust by hiring a foster-mother, or by feeding on artificial food, once prevalent among the wealthy classes, today already stigmatized as unnatural. Here, again, the sociologist has to add his share in order to give the true picture of motherhood, as it varies according to national, economic, and moral differences.

Let us now turn to consider the same relation in a matrilineal society on the shores of the Pacific. The Melanesian mother shows invariably a passionate craving for her child, and the surrounding society seconds her feelings and fosters by custom and usage her inclinations and idealizes them. From the first moments of pregnancy, the mother is made to watch over the welfare of her offspring by keeping a number of food taboos and other observances. The pregnant woman is regarded by custom as an object to be revered, an ideal which is fully realized by the actual behavior and feelings of these natives. There exists an elaborate ceremony performed at the first pregnancy, with an intricate and somewhat obscure aim, but emphasizing the importance of the event and conferring on the pregnant one distinction and honor.

After the birth, mother and child are secluded for about a month, the mother constantly tending her child and nursing it, while certain female relatives only are admitted into the hut. Adoption under normal circumstances is very rare, and even then the child is usually given over only after it has been weaned, nor is it ever adopted by strangers, but by nearest relatives exclusively. A number of observances, such as ritual washing of mother and child, special taboos to be kept by the mother, and visits of presentation bind mother and child by links of custom superimposed upon the natural ones.

Thus in both societies, to the biological adjustment there are added the social forces of custom, morals, and manners, both working in the same direction of binding mother and child to each other, of giving them full scope for the passionate intimacy of motherhood. This harmony between social and biological forces ensures full satisfaction and highest bliss.

We find a much greater difference in the fatherhood of the patriarchal and matrilineal family at this period, and it is rather unexpected to find that in a savage society, where the physical bonds of paternity are unknown, and where mother-right obtains, the father should yet stand in a much more intimate relation to the children than normally happens among ourselves. For in our society, the father plays a very small part indeed with a young infant. By custom, usage, and manners, the well-to-do father is kept out of the nursery, while the peasant or working man has to leave the child to his wife for the greater part of the 24 hours. He may perhaps resent the attention which the infant claims, and the time which it takes up, but as a rule he neither helps nor interferes with the child. Among the Melanesians, "fatherhood," as we know, is a purely social relation. Now part of this relation consists in his duty toward his wife's children; he is there "to receive them into his arms," a phrase we have already quoted; he has to carry them about when on the march the mother is tired, and he has to assist in the nursing at home. He tends them in their natural needs, and cleanses them, and there are many stereotyped expressions in the native language referring to fatherhood and its hardships, and to the duty of filial gratitude toward him. A typical Trobriand father is a hard-working and conscientious nurse, in which he obeys the call of duty, expressed in social tradition. The fact is, however, that the father is always interested in the children, sometimes passionately so, and performs all his duties eagerly and fondly. Thus if we compare the patriarchal and the matrilineal relation at this early stage, we see that the main point of difference lies with the father. In our society, the father is well kept out of the picture, and has at best a subordinate part. In the Trobriands, he plays a much more active role, which is important above all, because it gives him a far greater scope for forming ties of affection toward his children. In both societies there is, with a few exceptions, little room for conflict found between the biological trend and the social conditions.

THE FIRST CONFLICT IN THE PATRIARCHAL AND THE CONTINUED HARMONY IN THE MATRILINEAL SOCIETIES

We have now reached the period when the child is already weaned, when it is learning to walk and begins to speak. Yet biologically it is gaining its independence from the mother's body but slowly. It clings to it with undiminished, passionate desire for her presence, for the touch of her body and the tender clasp of her arms.

This is the natural, biological tendency, but in our society, at one stage or another, the child's desires are crossed and thwarted. Let us first realize that this epoch is introduced by the process of weaning. By this the blissful harmony of infantile life is broken, or at least modified. Among the higher classes, weaning is so prepared, graduated, and adjusted that it usually passes without any shock. But among women of the lower classes, in our society, weaning is often a painful wrench for the mother and certainly for the child. Later on, other obstacles tend to obtrude upon the intimacy of mother and child, in whom at that stage a notable change is taking place. He becomes independent in his movements, can feed himself, express some of his feelings and ideas, and begins to understand and observe. In the higher classes, the nursery arrangements separate the mother from the child in a gradual manner. This dispenses with any shock, but it leaves a gap in the child's life, a yearning and an unsatisfied need. In the lower classes where the child shares the parent's bed, it becomes at a certain time a source of embarrassment and an encumbrance, and it suffers a rude and more brutal repulsion.

How does savage motherhood in the coral islands of New Guinea compare at this stage with ours? First of all, weaning takes place much later in life, at a time when a child is already independent, can run about, eat practically everything and follow other interests. It takes place, that is, at a moment when the child neither wants nor needs the mother's breast any more, so that the first wrench is eliminated.

"Matriarchate," the rule of the mother, does not in any way entail a stern, terrible mother virago. The Trobriand mother carries her children, fondles them, and plays with them at this stage quite as lovingly as at the earlier period and custom as well as morals expect it from her. The child is bound to her, also, according to law, custom, and usage, by a closer tie than her husband is, whose rights are subservient to those of the offspring. The alcove psychology of the marital relations has therefore a different character and the repulsion of the child from the mother by the father is certainly not a typical occurrence, if it ever occurs at all. Another difference between her role and that of the typical European mother is that in Melanesia she is much more indulgent. Since there is little training of the child to do, and hardly any moral education, and what there is begins later and is done by other people, there is scarcely room for severity. This absence of maternal discipline precludes on the one hand such aberrations of severity as are sometimes found among us; on the other hand, however, it lessens a feeling of interest on the part of the child, a desire to please the mother, to win her approval, which is one of the strong links of filial attachment among us, and one which holds great possibilities for the establishment of a permanent relation in later life.

Turning now to the paternal relation we see that, in our society, irrespective of nation or social layer, the father still enjoys the patriarchal status. He is the head of the family and the relevant link in the lineage, and he is also the economic provider. As an absolute ruler of the family, he is liable to become a tyrant, in which case frictions of all sorts arise between him and his wife and children. The details of these depend greatly on the social milieu. In the wealthy classes of Western civilization, the child is well separated from his father by all sorts of nursery arrangements. Although constantly with the nurse, the child is usually attented to, and controlled by, the mother, who, in such cases, almost invariably takes the dominant place in the child's affections. The father, on the other hand, is seldom brought within the child's horizon, and then only as an onlooker and stranger, before whom the children have to behave themselves, show off and perform. He is the source of authority, the origin of punishment, and therefore becomes a bogey. Usually the result is a mixture: He is the perfect being for whose benefit the best of everything has to be done; and, at the same time, he is the ogre whom the child has to fear and for whose comfort, as the child soon realizes, the household is arranged. The loving and sympathetic father will assume the former rôle of a demigod. The pompous, wooden, or tactless one will soon earn the suspicion and even hate of the nursery. In relation to the father, the mother becomes an intermediary who is sometimes ready to denounce the child to the higher authority, but who at the same time can intercede against punishment.

The picture is different, though the results are not dissimilar, in the one-room and one-bed household of the poor peasantry of Central and Eastern Europe, or of the lower working classes. The father is brought into closer contact with the child, which in rare circumstances allows of a greater affection, but usually gives rise to much more acute and chronic friction. When a father returns home tired from work, or drunk from the inn, he naturally vents his ill-temper on the family, and bullies mother and children. There is not a village, no poor quarter in a modern town, where cases could not be found of sheer, patriarchal cruelty. From my own memory, I could quote numerous cases when peasant fathers would, on returning home drunk, beat the children for sheer pleasure, or drag them out of bed and send them into the cold night.

Even at the best, when the working father returns home, the children have to keep quiet, stop rowdy games, and repress spontaneous, childish outbursts of joy and sorrow. The father is a supreme source of punishment in poor households also, while the mother acts as intercessor, and often shares in the treatment meted out to the children. In the poorer households, besides the economic role of provider and the social power of the father are more quickly and definitely recognized, and act in the same direction as his personal influence.

The role of the Melanesian father at this stage is very different from that of the European patriarch. I have briefly sketched his very different social position as husband and father, and the part he plays in the household. He is not the head of the family, he does not transmit his lineage to his children, nor is he the main provider of food. This entirely changes his legal rights and his personal attitude to his wife. A Trobriand man will seldom quarrel with his wife, hardly ever attempt to brutalize her, and he will never be able to exert a permanent tyranny. Even sexual cohabitation is not regarded by native law and usage as the wife's duty and the husband's privilege as is the case in our society. The Trobriand natives take the view, dictated by tradition, that the husband is indebted to his wife for sexual services, that he has to deserve them and pay for them. One of the ways, the chief way in fact, of acquitting himself of this duty is by performing services for her children and showing affection to them. There are many native sayings which embody in a sort of loose folk-lore these principles. In the child's infancy the husband has been the nurse, tender and loving; later on in early childhood he plays with it, carries it, and teaches such amusing sports and occupations as take its fancy.

Thus the legal, moral, and customary tradition of the tribe and all the forces of organization combine to give the man, in his conjugal and paternal role, an entirely different attitude from that of a patriarch. And though it has to be defined in an abstract manner, this is by no means a mere legal principle, detached from life. It expresses itself in every detail of daily existence and permeates the whole relations within the family, and dominates the sentiments found there. The children never see their mother subjugated or brutalized or in abject dependence upon her husband, not even when she is a commoner married to a chief. They never feel his heavy hand on themselves; he is not their kinsman, not their owner, nor their benefactor. He has no rights or prerogatives. Yet he feels, as does every normal father all over the world, a strong affection for them and this, together with his traditional duties, makes him try to win their love, and thus to retain his influence over them.

Comparing European with Melanesian paternity, it is important to keep the biological fact in mind as well as the sociological. Biologically there is undoubtedly in the average man a tendency toward affectionate and tender feelings for his children. But this tendency seems not to be strong enough to outweigh the many hardships which children entail on a parent. When therefore society steps in and in one case declares that the father is the absolute master, and that the children should be there for his benefit, pleasure, and glory, this social influence tilts the balance against a happy equilibrium of natural affection and natural impatience of the nuisance. When, on the other hand, a matrilineal society grants the father no privileges and no rights to his children's affections, then he must earn them, and when again, in the same uncivilized society, there are fewer strains on his nerves and his ambitions and his economic responsibilities, he is freer to give himself up to paternal feelings. Thus in our society the adjustment between biological and social forces, which was satisfactory in the earliest period, begins to show a lack of harmony in this second one. In the Melanesian Society, the harmonious proportions remain. Father-right, we have seen, is to a great extent a source of family conflict, in that it grants to the father social claims and prerogatives not commensurate to his biological propensities, nor to the personal affection which he can feel for and arouse in his children.

INFANTILE SEXUALITY OF SAVAGE AND CIVILIZED CHILDREN

Traversing the same ground as Freud and the psychoanalysts, I have yet tried to keep clear of the subject of the sex, partly in order to avoid moot thoretical distinctions as to the nature of mother and child attachment or "libido." But at this stage, as the children begin to play independently and develop an interest in the surrounding work and people, sexuality makes its first appearance in forms accessible to outside sociological observation and affecting family life directly. A careful observer of European children, and one who has not forgotten his own childhood, has to recognize that at an early age, say, between three and four, there arises in them a special sort of interest and curiosity. Besides the world of lawful, normal, and nice things, there opens up a world of shame-faced desires, clandestine interests, and subterranean impulses. The two categories of things, "decent" and "indecent," "pure" and "impure" begin to crystallize, categories destined to remain throughout life. In some people the "indecent" becomes completely suppressed, and the right values of decency become hypertrophied into the virulent virtue of the puritan, or the still more repulsive hypocrisy of the conventionally moral. Or the decent is altogether smothered through glut in pornographic satisfaction, and the other category develops into a complete pruriency of mind, not less repulsive than hypocritical virtue itself.

In the second stage of childhood which we are now considering, according to my scheme about four to six, that is, the "indecent" centers round interest in excretory functions, exhibitionism, games with indecent exposure, often associated with cruelty. It hardly differentiates between the sexes, and is little interested in the genital side of matters. Anyone who has lived for a long time among peasants and knows intimately their juvenile ways will recognize that this state of affairs exists as a thing normal, though not open. Among the working classes things seem to be similar. Among the higher classes, conditions are much more supressed, but not very different. Observations in these social strata, which would be more difficult than among peasants, should, however, be urgently carried out for pedagogical, moral, and eugenic reasons, and suitable methods of research devised. The results would, I think, confirm to an extraordinary degree some of the assertions of Freud and his school.

How does the newly awakened infantile sexuality or infantile indecency influence the relation to the family? In the division between things "decent" and "indecent," the parents, and especially the mother, are included wholly within the first category, and remain in the child's mind absolutely untouched by the "indecent." The feeling that the mother might be aware of any prurient infantile play is extremely distasteful to the child, and there is a strong disinclination to allude in her presence to, or to speak with her about, any sexual matters. The father, who is also kept strictly outside the "indecent" category is, moreover, regarded as the moral authority whom these thoughts and pastimes would offend. For the "indecent" always carries with it a sense of guilt.

Freud and the psychoanalytic school have laid great stress on the sexual rivalry between mother and daughter, father and son respectively. My own opinion is that the rivalry between mother and daughter does not begin at this early stage. At any rate, I have never observed any traces of it. The relations between father and son are more complex. Although, as I have said, the little boy has no thoughts, desires, or impulses toward his mother which he himself would feel belong to the category of the "indecent," there can be no doubt that a young organism reacts sexually to close bodily contact with the mother. A well-known piece of advice given by old gossips to young mothers in peasant communities is to the effect that boys above the age of three should sleep separately from the mother. The occurance of infantile erections is well known in these communities, and the fact that the boy clings to the mother in a different way from the girl. That the father and the young male child have a component of sexual rivalry under such conditions seems even to an outside sociological observer probable. The psychoanalysts maintain it categorically. Among the wealthier classes crude conflicts arise more seldom, if ever, but they arise in imagination and in a more refined though perhaps not less insidious form.

It must be noted that at this stage when the child begins to show a different character and temperament according to sex, the parents' feelings are differentiated between sons and daughters. The father sees in the son his successor, the one who is to replace him in the family lineage and in the household. He becomes therefore all the more critical, and this influences his feelings in two directions; if the boy shows signs of mental or physical deficiency, if he is not up to the type of the idea in which the father believes, he will be a source of bitter disappointment and hostility. On the other hand, even at that stage, a certain amount of rivalry, the resentment of future supersession, the melancholy of the waning generation lead again to hostility. Repressed in both, this hostility hardens the father to the son and provokes by reaction a response in hostile feelings. The mother, on the other hand, has no grounds for negative sentiments, and has an additional admiration for the son as a man to be. The father's feeling toward the daughter—a repetition of himself in a feminine form—hardly fails to evoke a tender emotion, and perhaps also to flatter his vanity. Thus, social factors mix with biological, make the father cling more tenderly to the daughter than to the son, while with the mother it is the reverse. But it must be noted that an attraction of the offspring of the other sex, because it is of the other sex, is not necessarily sexual attraction.

In Melanesia, we find an altogether different type of sexual development in the child. That the biological impulses do not differ essentially, seems beyond a doubt. But I have failed to find any traces of what could be called infantile indecencies, or of a subterranean world in which children would indulge in clandestine pastimes centering around excretory functions or exhibitionism. The subject presents naturally certain difficulties of observation, for it is hard to enter into any personal communication with a savage child, and if there were a world of indecent things as amongst ourselves, it would be as futile to enquire about it from an average grown-up native as from a conventional mother, father, or nurse in our society. But there is one circumstance which makes matters so entirely different among these natives, that there is no danger of making a mistake. That is, that with them, there is no repression, no censure, no moral reprobation of infantile sexuality of the genital type when it comes to light at a somewhat later stage than the one we are now considering—at about the age of five or six. So if there had been any earlier indecency, this could be as easy observed as the later genital sign of sexual plays.

How can we then explain that among savages, there is no period of what Freud calls pregenital, anal-erotic interest? We shall be able to understand this better when we discuss the sexuality of the next stage in the child's development, a sexuality in which native Melanesian children differ essentially from our own.

APPRENTICESHIP TO LIFE AND REACTION TO AUTHORITY

We enter now on the third stage of childhood commencing between the ages of five and seven. At this period, a child begins to feel independent, to create its own games, to seek for associates of the same age, with whom it has a tendency to roam about unencumbered by grown-up people. This is the time when play begins to pass into more definite occupations and serious life interests.

Let us follow our parallel at this stage. In Europe, entrance into school or, among the uneducated classes, some sort of preliminary apprenticeship to an economic occupation removes the child from the influence of the family. The boy and girl lose to some extent their exclusive attachment to the mother. With the boy, there frequently happens at this period a transference of sentiment to a substitute mother, who, for the time being, is regarded with some of the same passionate tenderness felt for the mother, but with no other feelings. Such transference must not be confused with the tendency of adolescent boys much later to fall in love with women older than themselves. At the same time, there arises a desire for independence from the all-possessive intimacy of maternal interest, which makes the child withhold its absolute confidence from the parent. Among the peasants and lower classes, the process of emancipation from the mother takes place earlier than in the higher, but they are similar in all essentials. When the mother is deeply attached to the child, especially to the boy, she is apt to feel a certain amount of jealousy and resentment at this emancipation and to put obstacles into its way. This usually makes the wrench only more painful and violent.

The children on the coral beaches of the Western Pacific show a similar tendency. This appears there even more clearly, for the absence of compulsory education and of any strict discipline at this age allow much freer play to the natural inclinations of infantile nature. On the part of the mother there is in Melanesia, however, no jealous resentment or anxiety at the child's new-found independence, and here we see the influence of the lack of any deep, educational interest between mother and child. At this stage, the children in the Trobriand archipelago begin to form a small, juvenile community within the community. They roam about in bands, play on distant beaches or in secluded parts of the jungle, join with other small communities of children from neighboring villages, and in all this, though they obey the commands of their child-leaders, they are almost completely independent of the elders' authority. The parents never try to keep them back, to interfere with them in any way, or to bind them to any routine. At first, of course, the family still retains a considerable hold over a child, but the process of emancipation progresses gradually and constantly in an untrammelled, natural manner.

In this there is a great difference between European conditions, where the child often passes from the intimacy of the family to the cold discipline of the school or other preliminary training, and the Melanesian state of affairs where the process of emancipation is gradual, free, and pleasant.

And now what about the father at this stage? In our society he still represents the principle of authority within the family. Outside, at school, in the workshop, in the preliminary manual labor which the child of peasants is often set to do, it is the father personally or his authority indirectly or his substitute who wields the power. In the higher classes at this stage, the very important process of conscious formation of paternal authority and of the father ideal takes place. The child begins now to comprehend what it had guessed and felt before—the father's established authority as the head of the family and his economic influence. The ideal of his infallability, wisdom, justice, and might is usually in different degrees and in different manners inculcated into the child by the mother or the nurse in religious and moral teaching. Now the rôle of an ideal is never an easy one, and to maintain it in the intimacy of daily life, especially for one whose bad tempers and follies are not repressed by any discipline, is a very difficult performance indeed. Thus no sooner is the father ideal formed than it begins to decompose. The child feels at first only a vague malaise at his father's bad temper or weakness, a fear of his wrath, a dim feeling of injustice, perhaps some shame when the father has a really bad outburst. Soon the typical father-sentiment is formed, full of contradictory emotions, a mixture of reverence, contempt, affection and dislike, tenderness and fear. It is at this period of childhood that the social influence due to patriarchal institutions makes itself felt in the child's attitude toward the male parent. Between the boy and his father, the rivalries of successor and superseded and the mutual jealousies described in the previous section crystallize more distinctly and make the negative elements of the father-to-son relation more predominant than with that of father-to-daughter.

Among the lower classes, the process of the idealization of the father is cruder but not less important. As I have already said, the father in a typical peasant household is openly a tyrant. The mother acquiesces in his supremacy and imparts the attitude to her children, who reverance and at the same time fear the strong and brutal force embodied in their father. Here also a sentiment composed of ambivalent emotion is formed, with a distinct preference of the father for his female children.

What is the father's rôle in Melanesia? Little need be said about it at this stage. He continues to befriend the children, to help them, to teach them what they like and as much as they like. Children, it is true, are less interested in him at this stage and prefer, on the whole, their small comrades. But the father is always there a helpful adviser, half playmate, half protector.

Yet at this period the principle of tribal law and authority, the submission to constraint and to the prohibition of certain desirable things enters the life of a young boy or girl. But this law and constraint are represented by quite another person than the father, by the mother's brother, the male head of the family in a matriarchal society. He it is who actually wields the power and who indeed makes ample use of it.

His authority, though closely parallel to that of the father among ourselves, is not exactly identical with it. First of all his influence is introduced into the child's life much later than that of the European father. Then again, he never enters the intimacy of family life, lives in another hut, often in a different village, for, since marriage is patrilocal in the Trobriands, his sister and her children have their abode in the village of the husband and father. Thus his power is exercised from a distance and it cannot become oppressive in those small matters which are most irksome. He brings into the life of the child, whether boy or girl, two elements: first of all, that of duty, prohibition and constraint; secondly, especially into the life of the boy, the elements of ambition, pride, and social values, half of that, in fact, which makes life worth living for the Trobriander. The constraint comes in, in so far that he begins to direct the boy's occupation, to require certain of his services and to teach him some of the tribal laws and prohibitions.

Two—Child Care in Samoa

MARGARET MEAD

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Mead is another anthropologist who has stressed the need for child psychology to have a perspective broader than the American home and the American nursery school. The following section is from Mead's first book, COMING OF AGE IN SAMOA.

Babies are always nursed, and in the few cases where the mother's milk fails her, a wet nurse is sought among the kinsfolk. From the first week they are also given other food, papaya, coconut milk, sugar-cane juice; the food is either masticated by the mother and then put into the baby's mouth on her finger, or if it is liquid, a piece of dark cloth is dipped into it and the child allowed to suck it, as shepherds feed orphaned lambs. The babies are nursed whenever they cry and there is no attempt at regularity. Unless a woman expects another child she will nurse a baby until it is two or three years old, as the simplest device for pacifying its crying. Babies sleep with their mothers as long as they are at the breast; after weaning they are usually handed over to the care of some younger girl in the household. They are bathed frequently with the juice of a wild orange and rubbed with coconut oil until their skins glisten.

The chief nursemaid is usually a child of six or seven who is not strong enough to lift a baby over six months old, but who can carry the child straddling the left hip, or on the small of the back. A child of six or seven months of age will assume this straddling position naturally when it is picked up. Their diminutive nurses do not encourage children to walk, as babies who can walk about are more complicated charges. They walk before they talk, but it is impossible to give the age of walking with any exactness, though I saw two babies walk whom I knew to be only nine months old, and my impression is that the average age is about a year. The life on the floor encourages crawling, and children under three or four years of age optionally crawl or walk.

From birth until the age of four or five a child's education is exceedingly simple. They must be housebroken, a matter made more difficult by an habitual indifference to the activities of very small children. They must learn to sit or crawl within the house and never to stand upright unless it is absolutely necessary; never to address an adult in a standing position; to stay out of the sun; not to tangle the strands of the weaver; not to scatter the cut-up coconut which is spread out to dry; to keep their scant loin cloths at least nominally fastened to their persons; to treat fire and knives with proper caution; not to touch the kava bowl, or the kava cup; and, if their father is a chief, not to crawl on his bed-place when he is by. These are really simply a series of avoidances, enforced by occasional cuffings and a deal of exasperated shouting and ineffectual conversation.

The weight of the punishment usually falls upon the next oldest child, who learns to shout, "Come out of the sun," before she has fully appreciated the necessity of doing so herself. By the time Samoan girls and boys have reached sixteen or seventeen years of age these perpetual admonitions to the younger ones have become an inseparable part of their conversation, a monotonous, irritated undercurrent to all their comments. I have known them to intersperse their remarks every two or three minutes with, "Keep still," "Sit still," "Keep your mouths shut," "Stop that noise," uttered quite mechanically although all of the little ones present may have been behaving as quietly as a row of intimidated mice. On the whole, this last requirement of silence is continually mentioned and never enforced. The little nurses are more interested in peace than in forming the characters of their small charges and when a child begins to howl, it is simply dragged out of earshot of its elders. No mother will ever exert herself to discipline a younger child if an older one can be made responsible.

If small families prevailed in Samoa, this system would result in making half of the population solicitous and self-sacrificing and the other half tyrannous and self-indulgent. But just as a child is getting old enough so that its wilfulness is becoming unbearable, a younger one is saddled upon it, and the whole process is repeated again, each child being disciplined and socialized through responsibility for a still younger one.

This fear of the disagreeable consequences resulting from a child's crying is so firmly fixed in the minds of the older children that long after there is any need for it, they succumb to some little tyrant's threat of making a scene, and the five-year-olds bully their way into expeditions on which they will have to be carried, into weaving parties where they will tangle the strands, and cook-houses where they will tear up the cooking leaves or get thoroughly smudged with the soot and have to be washed—all because an older boy or girl has become so accustomed to yielding any point to stop an outcry.

This method of giving in, coaxing, bribing, diverting the infant disturbers is only pursued within the household or the relationship group, where there are duly constituted elders in authority to punish the older children who can't keep the babies still. Toward a neighbor's children or in a crowd the half-grown girls and boys and even the adults vent their full irritation upon the heads of troublesome children. If a crowd of children are near enough, pressing in curiously to watch some spectacle at which they are not wanted, they are soundly lashed with palm leaves, or dispersed with a shower of small stones, of which the house floor always furnishes a ready supply. This treatment does not seem actually to improve the children's behavior, but merely to make them cling even closer to their frightened and indulgent little guardians. It may be surmised that stoning the children from next door provides a most necessary outlet for those who have spent so many weary hours placating their own young relatives. And even these bursts of anger are nine-tenths gesture. No one who throws the stones actually means to hit a child, but the children know that if they repeat their intrusions too often, by the laws of chance some of the flying bits of coral will land in their faces. Even Samoan dogs have learned to estimate the proportion of gesture that there is in a Samoan's "get out of the house." They simply stalk out between one set of posts and with equal dignity and all casualness stalk in at the next opening.

By the time a child is six or seven she has all the essential avoidances well enough by heart to be trusted with the care of a younger child. And she also develops a number of simple techniques. She learns to weave firm square balls from palm leaves, to make pinwheels of palm leaves or frangipani blossoms, to climb a coconut tree by walking up the trunk on flexible little feet, to break open a coconut with one firm well-directed blow of a knife as long as she is tall, to play a number of group games and sing the songs which go with them, to tidy the house by picking up the litter on the stony floor, to bring water from the sea, to spread out the copra to dry and to help gather it in when rain threatens, to roll the pandanus leaves for weaving, to go to a neighboring house and bring back a lighted fagot for the chief's pipe or the cook-house fire, and to exercise tact in begging slight favors from relatives.

But in the case of the little girls all of these tasks are merely supplementary to the main business of baby-tending. Very small boys also have some care of the younger children, but at eight or nine years of age they are usually relieved of it. Whatever rough edges have not been smoothed off by this responsibility for younger children are worn off by their contact with older boys. For little boys are admitted to interesting and important activities only so long as their behavior is circumspect and helpful. Where small girls are brusquely pushed aside, small boys will be patiently tolerated and they become adept at making themselves useful. The four or five little boys who all wish to assist at the important business of helping a grown youth lasso reef eels, organize themselves into a highly efficient working team; one boy holds the bait, another holds an extra lasso, others poke eagerly about in holes in the reef looking for prey, while still another tucks the captured eels into his *lavalava.* The small girls, burdened with heavy babies or the care of little staggerers who are too small to adventure on the reef, discouraged by the hostility of the small boys and the scorn of the older ones, have little opportunity for learning the more adventurous forms of work and play. So while the little boys first undergo the chastening effects of baby-tending and then have many opportunities to learn effective cooperation under the supervision of older boys, the girls' education is less comprehensive. They have a high standard of individual responsibility but the community provides them with no lessons in cooperation with one another. This is particularly apparent in the activities of the young people; the boys organize quickly; the girls waste hours in bickering, innocent of any technique for quick and efficient cooperation.

And as the woman who goes fishing can only get away by turning the babies over to the little girls of the household, the little girls cannot accompany their aunts and mothers. So they learn even the simple processes of reef fishing much later than do the boys. They are kept at the baby-tending, errand-running stage until they are old enough and robust enough to work on the plantations and carry foodstuffs down to the village.

A girl is given these more strenuous tasks near the age of puberty, but it is purely a question of her physical size and ability to take responsibility, rather than of her physical maturity. Before this time she has occasionally accompanied the older members of the family to the plantations if they were willing to take the babies along also. But once there, while her brothers and cousins are collecting cocoanuts and roving happily about in the bush, she has to chase and shepherd and pacify the ubiquitous babies.

As soon as the girls are strong enough to carry heavy loads, it pays the family to shift the responsibility for the little children to the younger girls and the adolescent girls are released from baby-tending. It may be said with some justice that the worst period of their lives is over. Never again will they be so incessantly at the beck and call of their elders, never again so tyrannized over by two-year-old tyrants. All the irritating, detailed routine of housekeeping, which in our civilization is accused of warping the souls and souring the tempers of grown women, is here performed by children under fourteen years of age. A fire or a pipe to be kindled, a call for a drink, a lamp to be lit, the baby's cry, the errand of the capricious adult—these haunt them from morning until night. With the introduction of several months a year of government schools these children are being taken out of their homes for most of the day. This brings about a complete disorganization of the native households which have no precedents for a manner of life where mothers have to stay at home and take care of their children and adults have to perform small routine tasks and run errands.

Three—Social Class and Color Differences in Child Rearing

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Recently much attention has been drawn to the fact that different social classes in America vary appreciably in culture. Considerable interest is now being shown in the process by which children of various subgroups in this country are reared, and attempts are being made to determine the consequences of these differences in child training. The investigation by Davis and Havighurst is a representative study.

In recent years, cultural anthropologists and social psychologists have made intensive studies of the relationships between personality and socialization. They have arrived at a methodological distinction which has proved helpful in the analysis of personality. In the light of their comparative data on the socialization of individual children in different societies, they have set up the operational principle that personality can best be studied in terms of two basic interacting systems of behavior.

One system of actions, feedings, and thoughts is (a) cultural. It is learned by the individual from his basic social groups: his family, his age groups, his sex group, his social class group, and so on. The other system of responses is (b) individual, or "idiosyncratic" or "private." It derives in part from genetic factors and in part from learning. These learned individual traits are responses to organic, affectional and chance factors, and likewise to the particular deviations of a child's training from the standard cultural training for his group.

The use of this hypothesis and its various derivative forms seems likely to bring some order into the chaotic field of personality studies. As a methodological distinction, it is useful both in the exploration of the life history, and likewise in the cross-sectional study of personality traits in a population. As a method of securing and organizing data, it has the virtue of directing the researcher's attention to increasingly *smaller systems* of behavior in his search for the formants of personality. As an hypothesis about the dynamics of personality, this distinction between the cultural and the individual "personalities" leads to the useful effort to understand "adjustment" and "maladjustment," or "normal" and "neurotic" behavior, in terms of the degree of "fit" between a person's *individual* motivation, and the *cultural* demands made upon him to adhere to those rôles and those traits approved by his particular society.

The research whose findings will be summarized below is concerned primarily with the cultural aspects of personality. In a later report on a second part of our research, the development of individuality in children will be approached through intensive studies of the development of siblings. In this report, however, our purpose is to describe differences in the *cultural training* of children whose families are of different social and cultural status.

The nature of social stratification in the United States, and the cultural patterns of American social classes have been defined in extensive studies by Professor W. Lloyd Warner and his colleagues (3). Furthermore, the powerful influence of this social class system upon the American educational system, and upon what the child learns in school has been documented in a summary of recent studies.

To students of learning, and especially to those who wish to study the processes of socialization, a detailed understanding of American social class cultures and motivational patterns is now a *sine qua non* of both research and therapy. For the social class of the child's family determines not only the neighborhood in which he lives and the play groups he will have, but also the basic cultural acts and goals toward which he will be trained. The social class system maintains cultural, economic, and social barriers which prevent intimate social intermixture between the slums, the Gold Coast, and the middle class. We now that human beings can learn their culture only *from other human beings,* who already know and exhibit that culture. Therefore, by setting up barriers to social participation, the American social class system actually prevents the vast majority of children of the working classes, or the slums, from learning any culture but that of their own groups. *Thus the pivotal meaning of social class to students* of human development is that it defines and systematizes different learning environments for children of different classes.

There are three major types of cultural systems in the United States. The first is *the general American system of cultural behaviors;* these include some form of the American language, certain broad similarities, such as wearing "clothing," living in "houses," using machinery and so on; the monogamous family; the prohibition of incest and murder; and certain democratic "ideals."

The other major cultural systems are *the social class cultures,* and *the ethnic group cultures.* So powerful are the social class cultures that they tend to influence all the *general American cultural behaviors.* It is a fact that the specific form of the American language used, or of clothes, or of food, or of house, or even the social definition of a monogamous relationship varies by social class.

It has been assumed generally that the basic *areas* of training young children were very similar in all social strata, including weaning, toilet-training, property-training, and the like. This seems to be true. Our purpose, however, was to determine to what extent *the methods, the timing,* and *the pace* of this early training differed in the various social classes. We attempted to make the same comparison with regard to the training demands in middle childhood.

Throughout this research, we have concieved of personality as an organization consisting of (a) learned responses, (habits, thoughts, feelings, values, goals) and (b) the genetic factors of organic irritability, growth tempo, fatigue rate, and so on.

The *cultural* aspects of personality are the responses learned in conformity with the incentives and demands of a human group (a family, a gang, a social class). The *individual* aspects of personality include genetic factors, and in addition all other traits which distinguish between men who have been trained in the same culture, but whose responses to this training have varied according to the particular conditions under which the training took place.

The primary questions which this research attacked were: What are the training demands exerted upon the white and Negro child in the lower class and the middle class in Chicago, and What is the extent of the difference in the time of beginning, the length of, and the other conditions surrounding the training?

A forerunner to this study is that made by John Anderson (1) for the White House Conference of 1930. Anderson found that child-rearing practices are related to social status. The present study investigates some of the differences and relates them systematically to a theory of social structure and the socialization of the individual.

PROCEDURE

The study consisted of holding guided interviews with mothers of young children, recording their responses on a schedule, and making a statistical analysis of the data from the schedules. All the mothers were residents of Chicago, and most of them lived on the South Side of Chicago.

There were 50 mothers in each of 4 groups, white middle class, white lower class, Negro middle class, and Negro lower class. Data on the ages of the mothers and fathers, the number of children, and the ages of their children are given in Table I.

TABLE I

DATA ON FAMILIES IN THE STUDY

	Middle Class		*Lower Class*	
	White	Negro	White	Negro
Number of mothers	48	50	52	50
Median age of mother at marriage	23	21	20	18
Median age of father at marriage	25	25	25	21
Median age of mother at interview	33	29	29	29
Median age of father at interview	35	33	33	32
Number of children at time of interview	107	109	167	184
Number of families with 2 children only	34	28	12	14
Number of families with 3 or more children	11	14	34	32
Median age of children at interview	4	4	6	6

The interviewers were five women, who were trained specifically for the interviewing task in several sessions with the authors of this study. The interview was a lengthy one, lasting usually from two to three hours and often taking place in two separate sessions.

The schedule for the interview was developed by one of the authors on the basis of previous work of this kind. It consisted of three main parts: The first and longest section dealt with the actual training of the child or children by the mother, and with individual differences in personality among the children in the family. The second section dealt with the mother's expectations concerning the occupation, education, and responsibilities and privileges of her children, with the child's regimen (meals, sleep, recreation) relations with father, and so on. The third section dealt with socio-economic data concerning the mother and father and their families.

The families were classified into middle and lower social classes by using data from the interview which have been found to be closely correlated with social class placement as defined and described by Warner and Lunt (3) and by Davis, Gardner, and Gardner (2).

The principal factors used in making the classification were occupation of parents and their siblings, education of parents, their siblings, and grandparents, property ownership, membership in churches and other associations, and section of the city. One of the authors discussed these data with the interviewer in each case, and made the classification. There was seldom any doubt as to the proper classification. For the Negro group, the criteria were parallel to those for the classification of the white families, but shifted systematically because of restrictions on opportunity for Negroes in American society. For example, where the occupation of mail carrier would have suggested lower-class status for a white man, it suggested middle-class status for a Negro.

Since all the mothers were native-born, the factor of foreign parentage was ruled out of the study to a considerable extent. Nevertheless, this factor did appear in the white lower-class sample to a limited degree, due to the fact that about a third of this group were of fairly recent foreign extraction, largely Italian, living in South Chicago. The remainder of the lower-class white group live in the Woodlawn and Hyde Park Areas, and are practically all of "old American" stock.

The middle-class sample is probably more representative of upper-middle than of lower-middle class people, with a high proportion of fathers in professions and managerial positions in business. The lower-class sample is definitely upper-lower rather than lower-lower class. The fathers were mainly steady, hard-working people at the semiskilled and skilled levels.

The sample was not secured by a random procedure. Rather, it consisted mainly of people who had children in certain nursery schools, some private, and some war nurseries supported mainly by public funds. The South Chicago group consisted mainly of people who lived in the neighborhood in which one of the interviewers had grown up. The Woodlawn lower-class group was obtained by calling at random in certain areas where housing was obviously poor, and passing from one family to another with whom the person being interviewed was acquainted. Any systematic bias introduced by these procedures lay probably in the direction of getting a middle-class group which had been subjected to the kind of teaching about child-rearing which is prevalent among middle-class people who send their children to nursery schools.

RESULTS

There are a large number of reliable differences between classes and between colors. The tables report the differences between classes and colors on certain parts of the schedule. Those differences which are statistically reliable are summarized in Tables V and VI.

Feeding, Weaning, and Toilet Training

Table II shows the median ages for various aspects of feeding and toilet training, while Table III shows the proportions of children in the various class and color groups who had certain kinds of feeding and toilet-training experience.

Strictness of Regime

There were several questions in the interview which explored the degree of strictness of the regime set up by parents. These questions dealt with daytime naps, time children are due in the house at night, and age of going to the movies alone; all things in which the impulses of the child sometimes run counter to parental desires. Table IV shows the results on these questions.

Age of Assuming Responsibility

A number of the questions dealt with the age at which certain things are expected, such as dressing oneself, helping with younger children, learning to cook and to sew; and with the age at which certain responsibilities would be permitted, such as crossing the street alone. In general these questions dealt with the theme of *assuming responsibility.* "At what age does your child assume responsibility for one thing or another?" was asked in a variety of forms. There is not space enough to report the results of this section in detail, but the reliable differences are summarized in Tables V and VI. In general, it may be said that middle-class children are expected by their parents to assume responsibility earlier than lower-class children are expected to assume similar responsibilities by their parents.

Summary of Class Differences and Color Differences

Table V summarizes the class differences which are statistically reliable at the 5 per cent level, while Table VI summarizes the color differences which are statistically reliable at the same level. It will be seen that the differences tend to go together; a letter (c) indicates that the finding contradicts the general tendency of the results. For example, the general tendency is for lower-class children to be

TABLE II

Median Ages in Months for Various Aspects of Feeding and Toilet Training

	White				*Negro*			
	Middle		Lower		Middle		Lower	
	N.	Median	N.	Median	N.	Median	N.	Median
Breast-feeding finished	75	3.8	114	4.9	88	8.5	159	9.4
Bottle-feeding finished	95	10.7	123	12.9	74	12.5	99	12.6
Sucking finished	99	10.5	147	12.8	104	12.0	177	12.2
Bowel training begun	99	7.5	158	10.2	105	5.5	172	8.5
Bowel training complete	91	18.4	152	18.8	95	13.4	160	18.6
Bladder training begun	93	11.2	156	12.2	102	9.2	124	11.1
Bladder training complete	81	24.6	139	24.0	76	18.0	143	19.0

TABLE III

Proportions of Children with Certain Kinds of Feeding and Toilet-Training Experience

	White				*Negro*			
	Middle		Lower		Middle		Lower	
	No/Total	*Per Cent*	*No/Total*	*Per Cent*	*No/Total*	*Per Cent*	*No/Total*	*Per Cent*
Children breast-fed only	5/106	5	28/163	17	32/107	30	80/179	45
Children bottle-fed only	31/100	31	49/151	32	19/106	18	20/179	11
Children both breast- and bottle-fed	63/ 99	64	90/147	61	58/104	56	84/174	48
Children breast-fed one month or more	76/106	72	118/163	72	90/107	83	164/179	92
Children breast-fed longer than 3 months	34/106	32	66/163	41	63/107	59	145/179	81
Children sucking longer than 12 months	21/ 99	21	66/147	45	32/104	31	51/177	29
Children fed when they seemed hungry	3/106	3	53/153	35	6/108	6	87/175	50
Children having pacifiers	1/107	1	22/167	13	8/105	7	17/184	9
Children held for bottle or breast-fed only	53/ 79	67	72/166	43	78/108	72	99/179	55
Children weaned sharply	20/101	20	23/154	15	7/105	7	39/182	21
Children who sucked thumb	54/105	51	30/166	18	50/104	48	54/183	30
Bowel training begun at 6 mo. or earlier	48/ 99	49	36/158	23	91/105	87	49/172	29
Bowel training complete at end of 12 mo.	25/ 91	28	31/152	21	46/ 95	49	37/160	23
Bladder training begun at 6 mo. or earlier	17/ 95	18	22/157	14	4/102	40	22/124	18
Bladder training complete at end of 18 mo.	26/ 81	32	67/139	48	51/ 76	67	73/143	51
No. of children who have masturbated	56/104	54	27/162	17	30/102	29	27/182	15

TABLE IV

MOTHERS' REPORTS ON STRICTNESS OF REGIME

	White		*Negro*	
	Middle *Per Cent*	Lower *Per Cent*	Middle *Per Cent*	Lower *Per Cent*
Do children take nap in daytime?				
Yes	89	52	86	63
No	9	46	12	21
Sometimes	2	2	2	16
Total No. answering	46	50	50	48
Age at which boys go to movie alone				
5-7 years	17	35	35	70
Over 8	83	65	52	30
Doesn't approve of movies for child	0	0	13	0
Total No. answering	23	40	23	40
Age at which girls go to movie alone				
5-7	14	45	0	5
Over 8	86	55	72	95
Doesn't approve of movies for child	0	0	28	30
Total No. answering	28	42	32	37
Time boys in house at night				
5-6 o'clock	59	33	31	2
7-8 o'clock	32	33	63	57
9-10 o'clock	5	26	0	41
At will	4	8	0	0
Children not allowed on street	0	0	6	0
Total No. answering	22	39	32	44
Time girls in house at night				
5-6 o'clock	68	33	35	8
7-8 o'clock	29	39	56	63
9-10 o'clock	3	23	0	26
At will	0	5	0	0
Children not allowed on street	0	0	9	3
Total No. answering	31	39	34	38

treated more permissively than middle-class children with respect to feeding and weaning. Contradictory to this tendency, however, more middle-class children are held for feeding.

Comparison of Class and Color Differences

In Table VII are summarized quantitatively the differences in the results which are statistically reliable at the 5 per cent level. All differences which are statistically reliable are also practically significant, for the small numbers of cases prevent us from securing reliable differences which are quantitatively small and therefore of little practical significance.

TABLE V

CLASS DIFFERENCES IN CHILD REARING

Feeding and Weaning

More lower-class children are breast-fed only.
More lower-class children breast-fed longer than 3 months (Negro only).
More lower-class children are fed at will.
Weaning takes place earlier (on the average) among middle-class children (white only).
More lower-class children suck longer than 12 months (white only).
More lower-class children have pacifiers (white only).
(c) More middle-class children are held for feeding.
(c) More lower-class are weaned sharply (Negro only).

Toilet Training

Bowel training is begun earlier (on the average) with middle-class children.
Bladder training is begun earlier (on the average) with middle-class children.
Bowel training is completed earlier by middle-class children (Negro only).
More middle-class parents begin bowel training at 6 months or earlier.
More middle-class parents begin bladder training at 6 months or earlier (Negro only).
More middle-class parents complete bowel training at 12 months or earlier (Negro only).
More middle-class parents complete bladder training at 18 months or earlier Negro only).
(c) More lower-class parents complete bladder training at 18 months or earlier (white only).

Father-Child Relations

Middle-class fathers spend more time with children.
Middle-class fathers spend more time in educational activities with children (teaching, reading, and taking for walks).
Lower-class fathers discipline children more (Negro only).

Occupational Expectations for Children

Middle class expect higher occupational status for children.

Educational Expectation (Length of Education)

More middle-class children expected to go to college.

Age of Assuming Responsibility

Middle class expect child to help at home earlier.
Middle-class girls cross street earlier (whites only).
(c) Lower-class boys and girls cross street earlier (Negro only).
Middle-class boys and girls expected to go downtown alone earlier.
Middle-class girls expected to help with younger children earlier.
Middle-class girls expected to begin to cook earlier (white only).
Middle-class girls expected to begin to sew earlier (white only).
Middle-class girls expected to do dishes earlier (Negro only).
(c) Lower-class children expected to get job after school earlier.
(c) Lower-class children expected to quit school and go to work earlier.

Strictness of Regime

Middle-class children take naps in daytime more frequently.
Lower-class boys and girls allowed at movies alone earlier.
Middle-class boys and girls in house at night earlier.

The procedure in making this table was to count all differences which were statistically reliable and independent. By independent differences we mean differences which could not be calculated from other differences already on our list. For example, if we note the

TABLE VI

COLOR DIFFERENCES IN CHILD REARING

Feeding and Weaning

More Negro children are breast-fed only.
More Negro children are breast-fed for 3 months or more.
More Negro children are fed at will (lower class only).
More Negro children have pacifiers (middle class only).
More white children are weaned sharply (middle class only).
Weaning takes place earlier (on the average) among white children (middle class only).
(c) More white children suck longer than 12 months (lower class only).

Toilet Training

Bowel training is begun earlier with Negro children.
Bladder training is begun earlier with Negro children.
Bowel training is completed earlier with Negro children (middle class only).
Bladder training is completed earlier with Negro children.
More Negro parents begin bowel training at 6 months or earlier (middle class only).
More Negro parents begin bladder training at 6 months or earlier (middle class only).
More Negro parents complete bowel training at 12 months or earlier (middle class only).
More Negro parents complete bladder training at 18 months or earlier (middle class only).

Father-Child Relations

White fathers spend more time with children (lower class only).
White fathers teach and play more with children (lower class only).
Negro fathers discipline children more (lower class only).

Educational Expectations (Length of Education)

More Negro children expected to go to college (lower class only).

Age of Assuming Responsibility

Negro boys and girls cross street earlier (lower class only).
(c) White girls cross street earlier (middle class only).
Negro boys go downtown alone earlier (lower class only).
Negro girls expected to dress selves earlier.
Negro girls expected to go to store earlier.
Negro girls expected to begin to cook earlier (lower class only).
(c) Negro children expected to quit school and go to work later.

Strictness of Regime

Negro boys allowed to go to movies alone earlier.
(c) White girls allowed to go to movies alone earlier.
White boys and girls in house at night earlier.

difference in percentages of parents in two groups who report their children as taking naps in the daytime, we do not include the difference between these same groups who report their children as not taking naps, since this difference depends on the first one. We listed all of the reliable and independent differences—which included differences in training procedure, symptoms of maladjustment, age and sex expectations; as well as education, property ownership, associational membership, and so on, of parents and their relatives. We then counted these differences with the results shown in Table VII. There were more class differences than color differences, and the class differences occurred approximately equally often in Negro and white groups.

We then put as many differences as possible into the categories of Table VII. Three categories of permissivity are summed up in the total Permissivity score. The category of Training for Assumption of Responsibility is not included in the Permissivity score, and it is scored in the opposite direction, a high score on Training for Assumption of Responsibility being analogous to a low score on Permissivity.

TABLE VII

SUMMARY OF CLASS AND COLOR DIFFERENCES

Number of Statistically Reliable Differences Between Classes and Between Colors

Class			*Color*		
White		*Negro*	*Middle Class*		*Lower Class*
50	between classes	52	37	between colors	39
	Permissivity (Total)				
	Middle Class			*Whites*	
2	more permissive	2	9	more permissive	5
13	less permissive	15	8	less permissive	6
	Food Training				
	Middle Class			*Whites*	
1	more permissive	2	0	more permissive	1
5	less permissive	3	5	less permissive	3
	Toilet Training				
	Middle Class			*Whites*	
1	more permissive	0	8	more permissive	3
3	less permissive	7	0	less permissive	0
	Regimen				
	Middle Class			*Whites*	
0	more permissive	0	1	more permissive	1
5	less permissive	5	3	less permissive	3
	Training for Responsibility				
	Middle Class			*Whites*	
8	more severe	6	1	more severe	0
2	less severe	4	4	less severe	8

Individual Personality Traits

In addition to studying class and color differences in child-rearing we attempted to secure information on the relations between children's experiences during training and their personality traits. This was done through questions about birth order, physical activity, generosity, jealousy, happiness, and other characteristics.

A number of difficulties prevented us from making a great deal out of this part of the study. Chief among these difficulties was the fact that mothers' testimony on personality traits of their children is somewhat ambiguous. For example, one mother may report a child selfish for behavior which would not be considered especially selfish by another mother. Again, mothers with several children will be able to make comparative statements, such as "this child was hard to train," or "this child was very generous," with more certainty of being right than mothers of one or two children. Another difficulty undoubtedly lay in the fact that personality traits are results of multiple causation. For example, the causes of aggressive behavior in one child may be quite different from the causes of apparently similar behavior in another child.

Despite these difficulties, we have succeeded in discovering a few statistically reliable relationships between children's early experience and their personality characteristics, as reported by these mothers.

Physical Activity Related to Personality Traits. Considering all families, *except those with only one child,* we attempted to find the relations between degree of physical activity when young and other characteristics. Physical activity was taken as an index of physical vigor, combined with a drive for exploration. Since the mothers were asked which child was active when young, and which was most quiet when young, we could compare the "most active" and the "most quiet" for other characteristics. The children who were most active when young, compared with those who were most quiet when young, tend to be most active now, most punished, more aggressive, and less neat.

Generosity Related to Other Personality Traits. When the children reported as "most generous" are compared with those reported as "most selfish" the following relationships occur. The "most generous" children tend to be the happiest, and to fight less. In the white but not the Negro group, the "most generous" children tend to have been more quiet as infants. There is no consistent relation between selfishness and neatness as reported by the mothers.

Birth Order and Personality

In studying the relation of birth order to personality traits, we were forced to content ourselves with a study of the differences between first and second children in middle-class families with only two children. It would not have been wise to group all second children together because those with younger siblings would be in a different family constellation from those without younger siblings. If birth order influences personality, it probably does so through placing children of different birth order in different emotional relationships with other members of the family. Consequently, since a second child in a family with only two children would be in quite a different situation than a second child in a family with several children, we restricted our study to families with two children. Among both white and Negro middle-class families with only two children, the following pattern emerges.

The first child tends to be more jealous, more selfish, and neater.

The second child tends to be happier, more generous, and more punished.

DISCUSSION

The answer is clear to the principal question which this study was designed to answer. There are considerable social class differences in child-rearing practices, and these differences are greater than the differences between Negroes and whites of the same social class.

Personality Implications of Social Class Differences in Child-Rearing

Middle-class families are more rigorous than lower-class families in their training of children for feeding and cleanliness habits. They generally begin training earlier. Furthermore, middle-class families place more emphasis on the early assumption of responsibility for the self and on individual achievement. Finally, middle-class families are less permissive than lower-class families in their regimen. They require their children to take naps at a later age, to be in the house at night earlier, and, in general, permit less free play of the impulses of their children.

Generalizing from the evidence presented in the tables, we would say that middle-class children are subjected earlier and more consistently to the influences which make a child an orderly, conscientious, responsible, and tame person. In the course of this training middle-class children probably suffer more frustration of their impulses.

In the light of these findings, the data with respect to thumb-sucking are interesting. Three times as many white middle-class children are reported to suck their thumbs as white lower-class children, and almost twice as many Negro middle-class children do likewise. Thumb-sucking is generally thought of as a response to frustration of the hunger drive, or of the drive to seek pleasure through sucking. Since middle-class children are fed less frequently and are weaned earlier, the higher incidence of thumb-sucking would be expected. The Negro middle-class children are treated much more permissively than the white middle-class children with respect to feeding and weaning, but much more rigorously with respect to toilet-training. Yet the proportion of Negro middle-class children reported as sucking their thumb is almost the same as the proportion of white middle-class children so reported. *Perhaps thumb-sucking is a response to frustration of any sort, rather than to frustration in the feeding area alone.*

The data with respect to masturbation are also of interest in this connection. Three times as many white middle-class as compared with lower-class children are reported as masturbating. Twice as many Negro middle-class children as compared with Negro lower-class children are reported as masturbating. The meaning of these findings is obscured by the possibility that some lower-class mothers may not have understood the question. Or it may be that some of them did not watch as carefully for masturbation as middle-class mothers do, or some lower-class mothers may have been more hesitant than middle-class mothers in admitting that their children followed this practice. Yet none of these explanations seems probable, and perhaps the data should be taken at their face value. Perhaps masturbation is much more common among middle-class infants than among lower-class infants. If this is true, it might be explained in terms of the hypothesis that masturbation is in part a palliative to frustration. Children who are frustrated more would masturbate more, according to the hypothesis.

It is a surprising fact that the middle-class mothers, in general, expected their children to assume responsibility earlier in the home, to help with the younger children, and to cook and sew at an earlier age. For it seems obvious that there is more actual need of the children's help in lower-class families, where the work of children to be cared for is greater and the mother has very little help with the housework. The explanation probably lies in a tendency on the part of middle-class people to train their children early for achievement and responsibility, while lower-class people train their children to take responsibility only after the child is old enough to make the effort of training pay substantial returns in the work the child will do. Middle-class parents can afford to use time to train children to dress themselves, help around the home, sew, cook, and so on, at such an early age that the children cannot repay this training effort by their actual performance, although they may repay it by adopting attitudes of self-achievement and responsibility.

In addition to training their children to take responsibility early and to adopt attitudes favorable to self-achievement, middle-class families attempt to curb those impulses of the child which would lead to poor health, waste of time, and bad moral habits, according to middle-class views. Therefore they require their children to take daytime naps longer, to come into the house at night earlier and they do not permit their children to go alone to movies at an early age. Nevertheless, they encourage their children to be venturesome in the more "constructive" activities, from the middle-class point of view, of going downtown alone to museums, department stores, dancing lessons and the like.

Whether the middle-class or the lower-class practices are preferable is, of course, largely a matter of private opinion. But it is significant that there is now a considerable body of scientific and lay judgment operating in the middle class to make child-rearing practices more permissive. It is contended that the orthodox middle-class practices make children too anxious, and frustrate them too much for the best mental health in later life. On the other hand, it may be contended that civilized life requires the individual to be tamed and to learn to take constructive control of his impulses. A certain degree of anxiety is valuable, in that it puts the individual on his toes to learn the lessons and meet the demands of modern society in order to win the very considerable rewards of modern civilized social life.

Our own view is that the better child-rearing practices can be drawn from both middle- and lower-class life and made into a combination which is superior to both of the norms as they emerge in this study.

Personality Implications of Color Differences in Child-Rearing

The striking thing about this study is that Negro and white middle-class families are so much alike, and that white and Negro lower-class families are so much alike. The likenesses hold for such characteristics as number of children, ages of parents when married, as well as child-rearing practices and expectations of children.

There are, however, some very interesting color differences. The major color differences are found in the areas of feeding and cleanliness training.

Negroes are much more permissive than whites in the feeding and weaning of their children. The difference is greater in the middle class. Negro babies have a markedly different feeding and weaning experience from white babies.

The situation is reversed with respect to toilet training. Here the Negro parents are much stricter than white parents, both in middle- and lower-class circles. For example, 87 per cent of Negro middle-class mothers said they commence bowel training at six months or earlier, compared with 49 per cent of white middle-class mothers;

and the comparable figures for bladder training are 40 and 18 per cent.

If feeding, weaning, and toilet training have much influence on the personality, we should expect systematic differences between Negro and white people *of the same social class*, though it is not all clear just what these differences should be, since one group is more rigorous in its training in one area while the other group is more rigorous in the other area.

There is another noticeable color difference. Negroes of both classes tend to give their girls an earlier training for responsibility in washing dishes, going to the store, and dressing themselves. This is probably traceable to the fact that Negroes of both classes have less outside help in the home than whites do and consequently the help of the girls is more urgently needed. It is noticeable, also, that middle-class Negro girls are not allowed to play across the street or to go to the movies alone as early as white middle-class girls. This may be due to the fact that most middle-class Negroes are forced to live in much less desirable neighborhoods, from their point of view, than those in which middle-class whites live.

Personality Implications of Intrafamily Differences

The questionnaire was designed to get information on personality characteristics of children as they might be related to birth order, training experience, and kinds of discipline used. Very few pronounced relationships appeared. This may have been due to several factors. Perhaps the interview method as we used it is not suited to getting information on individual personality characteristics. Again, perhaps such relations as exist are too complicated to be seen clearly in a study like this with a relatively small number of subjects. Nevertheless, there were a few interesting intrafamily relationships.

For instance, the relation of "activity when young" to other characteristics is of considerable importance, since the degree of physical activity when young may be taken as an index of native vitality and of whatever inborn drive there may be for exploration or for physical activity. As we should expect, those "most active when young" were reported as most frequently punished, and as most active now. They were also reported as fighting most now, and as least neat. They were reported as happiest, except in the case of the white middle class. In general, it appears that various types of expressive, impulsive behavior tend to go together, and to characterize the happy child. An exception must be made of the white middle class, where happiness is reported by the mothers as associated with quietness rather than activity in the young child.

There are only a few characteristics clearly related to generosity and selfishness, though happiness and absence of fighting seem to be tied up with generosity. On the basis of simple Freudian principles one might expect selfishness and neatness to go together, but this hypothesis is not borne out by the data.

The data on birth order in relation to personality characteristics show some interesting trends. The first child in middle-class families of two children tends to be more jealous and more selfish than the second child. This may be taken as evidence in favor of the hypothesis of downward sibling rivalry as strongly influential in personality formation. Still, it is well to remember that mothers of young children, when there are only two in the family, may report the older as more jealous and selfish merely because the older is bigger and more able to assert himself. The second child was reported as happier and more generous, and also as more punished.

SUMMARY

This study has given clear evidence of the following things:

1. There are significant differences in child-rearing practices between the middle and lower social classes in a large city. The same type of differences exist between middle- and lower-class Negroes as between middle- and lower-class whites.

2. Middle-class parents are more rigorous than lower-class parents in their training of children for feeding and cleanliness habits. They also expect their children to take responsibility for themselves earlier than lower-class parents do. Middle-class parents place their children under a stricter regimen, with more frustration of their impulses, than do lower-class parents.

3. In addition to these social class differences, there are some differences between Negroes and whites in their child-rearing practices. Negroes are more permissive than whites in the feeding and weaning of their children, but they are much more rigorous than whites in toilet training.

4. Thus there are *cultural differences* in the personality formation of middle-class compared with lower-class people, *regardless of color,* due to their early training. And for the same reason there should be further but less marked cultural differences between Negroes and whites of the same social class.

5. In addition to the cultural differences between individuals due to early training experience, there are individual personality

differences between children in the same family. These are probably due to physiological differences and to differences in emotional relationships with other members of the family.

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Four—The Effects of Social Climates

KURT LEWIN

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The student is probably already familiar at second hand with the studies of social climates conducted by Lippitt and others under the direction of Lewin. The following is a nontechnical account prepared by the director of these studies.

The psychologist of today recognizes that there are few problems more important for the development of the child and the problem of adolescence than a study of the processes by which a child takes over or becomes opposed to the ideology and the style of living predominant in his social climate, the forces which make him belong to certain groups, or which determine his social status and his security within those groups.

A genuine attempt to approach these problems experimentally —for instance, that of social status or leadership—implies technically that one has to create different types of groups and to set up experimentally a variety of social factors which might shift this status. The experimental social psychologist will have to acquaint himself with the task of experimentally creating groups, creating a social climate or style of living. The sociologist I hope will therefore forgive him when he cannot avoid handling also the so-called sociological problems of groups and group life. Perhaps the social psychologist might prove to be even of considerable help to the sociologist. Frequently the investigation on the border line between two sciences has proved to be particularly fruitful for the progress of both of them.

Take, for instance, the concept "social group." There has been much discussion about how to define a group. The group often has been considered as something more than the sum of the individuals, something better and higher. One has attributed to it a "group mind." The opponents of this opinion have declared the concept of "group mind" to be mere metaphysics and that in reality the group is nothing other than the sum of the individuals.

To one who has watched the development of the concept of organism, whole, or Gestalt, in psychology this argumentation sounds strangely familiar. In the beginning of Gestalt theory, at the time of Ehrenfels, one attributed to a psychological whole, such as a melody, a so-called Gestalt quality—that is, an additional entity like a group mind, which the whole was supposed to have in addition to the sum of its parts. Today we know that we do not need to assume a mystical Gestalt quality, but that any dynamical whole has properties of its own. The whole might be symmetric in spite of its parts being asymmetric, a whole might be unstable in spite of its parts being stable in themselves.

As far as I can see, the discussion regarding group versus individual in sociology follows a similar trend. Groups are sociological wholes; the unity of these sociological wholes can be defined operationally in the same way as a unity of any other dynamic whole, namely, by the interdependence of its parts. Such a definition takes mysticism out of the group conception and brings the problem down to a thoroughly empirical and testable basis. At the same time it means a full recognition of the fact that properties of a social group, such as its organization, its stability, its goals, are something different from the organization, the stability, and the goals of the individuals in it.

How, then, should one describe a group? Let us discuss the effect of democratic, autocratic, and laissez-faire atmospheres or clubs which have been experimentally created by Lippitt (2, 3, 4), and Lippitt and White (1), at the Iowa Child Welfare Research Station. Let us assume the club had five members and five observers were available. It might seem the simplest way always to assign one observer to one member of the club. However, the result at best would be five parallel microbiographies of five individuals. This procedure would not yield a satisfactory record even of such simple facts of the group life as its organization, its subgroups, and its leader-member relationship, not to speak of such important facts as the general atmosphere. Therefore, instead of assigning every observer to one individual, one observer was assigned to record from minute to minute the organization of the group into subgroups, another the social interactions, and so on. In other words, instead of observing the properties of individuals, the properties of the group as such were observed.

It is well known that the amount of success a teacher has in the classroom depends not only on her skill but to a great extent on the atmosphere she creates. This atmosphere is something intangible; it is a property of the social situation as a whole, and might be measured scientifically if approached from this angle. As a beginning, therefore, Lippitt selected a comparison between a democratic and an autocratic atmosphere for his study. The purpose of his experiment was not to duplicate any given autocracy or democracy or to study an "ideal" autocracy or democracy, but to create setups which would give insight into the underlying group dynamics. Two groups of boys and girls, ten and eleven years of age, were chosen for a mask-making club from a group of eager volunteers of two different school classes. With the help of the Moreno test both groups were equated as much as possible on such qualities as leadership and interpersonal relations. There were 11 meetings of the groups, the democratic group meeting always two days ahead of the autocratic one. The democratic group chose its activities freely. Whatever they chose the autocratic group was then ordered to do. In this way the activities of the group were equated. On the whole, then, everything was kept constant except the group atmosphere.

The leader in both groups was an adult student. He tried to create the different atmospheres by using the following technique:

Democratic	*Authoritarian*
1. All policies a matter of group determination, encouraged and drawn out by the leader.	1. All determination of policy by the strongest person (leader).
2. Activity perspective given by an explanation of the general steps of the process during discussion at first meeting (clay mold, plaster of Paris, papier-mache, etc.). Where technical advice was needed, the leader tried to point out two or three alternative procedures from which choice could be made.	2. Techniques and steps of attaining the goal (completed mask) dictated by the authority, one at a time, so that future direction was always uncertain to a large degree.
3. The members were free to work with whomever they chose and the division of tasks was left up to the group.	3. The authority usually determined autocratically what each member should do and with whom he should work.
4. The leader attempted to be a group member in spirit and in in discussion but not to perform	4. The dominator criticized and praised individual's activities without giving objective rea-

Democratic	*Authoritarian*
much of the actual work. He gave objective praise and criticism.	sons and remained aloof from active group participation. He was always impersonal rather than outwardly hostile or friendly (a necessary concession in method).

During the meetings of the two groups, the observers noted the number of incidents and actions per unit of time. It was observed that the autocratic leader put forth about twice as much action toward the members as the democratic leader, namely, 8.4 actions as against 4.5. This difference is even greater if one takes into account only the initiated social approach, namely, 5.2 as against 2.1. Still greater is this difference in relation to ascendant or initiated ascendant behavior: The ascendant actions of the autocratic leader were nearly three times as frequent as those of the democratic leader.

In regard to submissive actions, the proportion was opposite, namely, more frequent by the democratic leader, although in both groups submissive actions of the leader were relatively rare. A similar relation held for the objective, matter-of-fact actions. Here too the democratic leader showed a higher frequency.

On the whole, then, there existed a much greater impact on the members of the group by the leader in autocracy than in democracy, and the approach was much more ascendant and less matter of fact.

When we attempt to answer the question "How does the leader compare with the ordinary member in an autocracy and a democracy?" we must refer to an ideal average member who is a statistical representation of what would happen if all activities were distributed equally among the members of the group, including the leader. In Lippitt's experiment the figures showed two facts clearly: In both groups the leader was really leading. The autocratic leader showed 118 per cent more initiated ascendant acts than the average ideal member, and the democratic leader 41 per cent more. Both leaders were less submissive than the average member, namely, the autocrat 78 per cent, the democrat 53 per cent. It was interesting to note that both showed also more matter-of-fact action than the average ideal member.

However, the difference between the ordinary member and the leader was much less pronounced in democracy than in autocracy, both in ascendant and submissive action. The democratic leader distinguished himself, also relatively, more by his greater matter-of-factness.

What do these figures indicate about the situation in which the autocratic and democratic group members find themselves? I can only mention a few aspects: In the autocratic group it is the leader who sets the policy. For instance, a child says: "I thought we decided to do the other mask." The leader answers: "No, *this* is the one *I* decided last time would be the best one." In dynamical terms such an incident means that the child would have been able to reach his own goal but the leader puts up a barrier against this locomotion. Instead he induces another goal for the child and a force in this direction. We are calling such goals, set up by the power of another person, an *induced* goal.

A parallel example in the democratic group might be this: A child asks, "How big will we make the mask? Are they out of clay or what?" The leader answers: "Would you like me to give you a little idea of how people generally make masks?" In other words, the leader in the democratic group, instead of hindering the children in getting to their own goal, bridges over whatever regions of difficulty that might exist. For the democratic group, many paths are open; for the autocratic only one, namely, that determined by the leader. In an autocracy the leader determines not only the kind of activity but also who should work with whom. In our experimental democracy all work cooperation was the result of spontaneous subgrouping of the children. In the autocracy 32 per cent of the work groups were initiated by the leader, as against no per cent in the democracy.

On the whole, then, the autocratic atmosphere gives a much greater and more aggressive dominance of the leader, and a narrowing down of the free movement of the members, together with a weakening of their power fields.

What is the effect of this atmosphere on the group life of the children? As measured by the observers the child-to-child relationship was rather different in the two atmospheres. There was about 30 times as much hostile domination in the autocracy as in the democracy, more demands for attention and much more hostile criticism; whereas in the democratic atmosphere cooperation and praise of the other fellow was much more frequent. In the democracy more constructive suggestions were made and a matter-of-fact or submissive behavior of member to member was more frequent.

In interpreting these data, we might say that the "style of living and thinking" initiated by the leader dominated the relations between the children. In the autocracy instead of a cooperative attitude, a hostile and highly personal attitude became prevalent. This was strikingly brought out by the amount of group or "we" feeling as against "I" feeling: Statements which were "We-centered" occurred twice as often in the democracy as in the autocracy, whereas far more statements in the autocracy were "I-centered" than in the democracy.

So far as the relation of the children toward the leader was concerned, the statistical analysis revealed that the children in the autocratic group who were *less submissive* to each other were about *twice* as submissive to their leader, as the children in the democratic group. Initiated approaches to the leader in the democratic group were less frequent than in the autocratic group. In autocracy the action by the member toward the leader had more the character of a response to an approach of the leader. The approach to the leader in the autocracy was more submissive, or kept at least on a matter-of-fact basis.

On the whole, then, the style of living in both atmospheres governed the child-child relation as well as the child-leader relation. In the autocratic group the children were less matter of fact, less cooperative, and submissive toward their equals, but more submissive to their superior than in the democracy.

Behind this difference of behavior lie a number of factors. The tension is greater in the autocratic atmosphere, and the dynamic structure of both groups is rather different. In an autocratic group there are two clearly distinguished levels of social status: The leader is the only one having higher status, the others being on an equally low level. A strong barrier kept up by the leader prevents anyone from increasing his status by acquiring leadership. In a democratic atmosphere the difference in social status is slight and there exists no barrier against acquiring leadership.

This has a rather clear effect on the amount of individuality. In our experiment every individual in the democracy showed a relatively greater individuality, having some field of his own in spite of the greater "we" feeling among them, or perhaps because of it. In the autocratic group on the contrary the children all had a low status without much individuality. The type of subgrouping showed this difference even more clearly. In the autocracy, there was little "we" feeling and relatively little spontaneous subgrouping among the children. If the work required the cooperation of four or five members, it was the leader who had to order the members to get together. In the democracy those groups came together spontaneously and they kept together about twice as long as in the autocracy. In the autocracy these larger units disintegrated much faster when left to themselves.

These group structures, in combination with the high tension in the autocracy, led in Lippitt's experiments to a *scapegoat* situation. The children in the autocratic group ganged together not against their leader, but against one of the children and treated him so badly that he ceased coming to the club. This happened to two different children during 12 sessions. Under autocratic rule any increase in status through leadership was blocked and the attempt to dominate was dictated by the style of living. In other words, every child became a potential enemy of every other one and the power fields of the children weakened each other, instead of strengthening each other by cooperation. Through combining in an attack against one individual the members who otherwise could not gain higher status were able to do so by violent suppression of one of their fellows.

One may ask whether these results are not due merely to individual differences. A number of facts rule out this explanation, although of course individual differences always play a rôle. Of particular interest was the transfer of one of the children from the autocratic to the democratic group, and of another from the democratic to the autocratic one. Before the transfer the difference between the two children was the same as between the two groups they belonged to, namely, the autocratic child was more dominating and less friendly and objective than the democratic one. However, after the transfer the behavior changed so that the previously autocratic child now became the less dominating and more friendly and objective child. In other words, the behavior of the children mirrored very quickly the atmosphere of the group in which they moved.

Later Lippitt and White studied four new clubs with other leaders. They included a third atmosphere, namely that of laissez-faire, and exposed the same children successively to a number of atmospheres. On the whole, the results bear out those of Lippitt. They show a striking difference between laissez-faire and democracy very much in favor of democracy. They show further two types of reaction in the autocratic groups, one characterized by aggression, the second by apathy.

On the whole, I think there is ample proof that the difference in behavior in autocratic, democratic, and laissez-faire situations is not a result of individual differences. There have been few experiences for me as impressive as seeing the expression in children's faces change during the first day of autocracy. The friendly, open, and cooperative group, full of life, became within a short half-hour a rather apathetic-looking gathering without initiative. The change from autocracy to democracy seemed to take somewhat more time than from democracy to autocracy. Autocracy is imposed upon the individual. Democracy he has to learn.

These experiments as a whole, then, bear out the observations of cultural anthropology and are well in line with other experiments on the effect of the situation as a whole. The social climate in which a child lives is for the child as important as the air it breathes. The group to which a child belongs is the ground on which he stands. His relation to this group and his status in it are the most important factors for his feeling of security or insecurity. No wonder that the group the person is a part of, and the culture in which he lives, determine to a very high degree his behavior and character. These social factors determine what space of free movement he has, and how far he can look ahead with some clarity into the future. In other words, they determine to a large degree his personal style of living and the direction and productivity of his planning.

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Five—Bringing up the Jewish Child

KURT LEWIN

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This article was written for a Jewish audience and appeared in a magazine for Jewish women. It is included here because, though it was written for a specific group, its treatment of the sociological necessity for group membership is one of the best that is available. As Lewin indicates, the principles involved are universal.

As the number of free countries keeps dwindling,[1] the attitudes and actions of the coming generation of Jews in the United States will be of the utmost importance for the Jewish people as a whole, in Europe and Palestine as well as in America. To a large degree, the actions will be determined by the attitudes which the growing children acquire. Therefore a realistic understanding by Jewish parents and teachers of the psychological and pedagogical problems involved is of the highest importance. We shall endeavor to clarify these problems by discussing the social setting which confronts the child. Only an education which takes stock of this setting can hope to be successful.

Let us consider a town of medium size in the Middle West containing a small Jewish group living in good relations with the Gentiles. The Jews, mainly middle-class people, while taking care of their own charity problems, cooperate fully in the economic, political, and social enterprises of the town. And it might even happen that a number of Gentiles contribute to Jewish charities.

There would not at first be many occasions where the non-Jewish environment makes the growing Jewish child feel he is classified as different from Gentile children. He might go through nursery school and kindergarten up to the fourth grade of elementary school before his first experience of being called "dirty Jew," perhaps on the occasion of a children's fight.

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[1] The student should note that this selection was written in 1940.

But when the growing child approaches the age where parents are accustomed to consider friendships between boys and girls as possibly leading to marriage, he is likely not to receive invitations to Gentile families for social occasions. However, the line is not always clearly and sharply defined; often it is doubtful whether the restriction is due to the child's being a Jew or to some other reason. It might happen that not until he attempts to secure employment does the young Jew personally encounter any severe restrictions.

But the Jewish boy or girl desiring to enter a university comes up against a definite classification. It is no secret that in most of the important universities an unofficial but nevertheless definite quota limits the number of Jews, particularly in professional schools. Only Jewish fraternities and sororities are open to Jewish students. Thus, his classification as a Jew receives an emphasis which often enough surpasses considerably the desires of the student.

How do the Jewish students react to this situation? There are any number who are quite able "to take it." That is, they show a well-adapted and balanced behavior, living happily and mingling with both Jewish and non-Jewish groups.

There are, however, a large number of Jewish students who show decided lack of adjustment. To one who has come to America from pre-Hitler Germany it is quite impressive to find such typical signs of Jewish maladjustment as overtension, loudness, overaggressiveness, excessively hard work—sometimes to an even higher degree here than over there.

What, then, are the factors that determine whether the individual Jew will show a balanced or an unbalanced behavior, and what should education do to secure balanced behavior?

One might be tempted to argue in this way: Anti-Semitism is only one of many difficulties the child is going to encounter. There are the ordinary difficulties with his friends. As a matter of fact, the occasions when a child is likely to meet with anti-Semitism, under the circumstances described, are rare compared with the frequency of the other problems. If, then, one looks upon the difficulties arising from anti-Semitism not from the point of view of the Jews as a group but from that of the single individual, could one not argue, even with some show of realism, that there need be no specific preparation of the child to meet anti-Semitism? Should it not suffice to strengthen the child's general ability to cope with difficulties, particularly his ability to take social setbacks? At least in early childhood, it could be argued, there seems to be no necessity for any specific preparation. It should be sufficient to help the child if and when the actual problems arise.

There are factors which favor such an attitude on the part of Jewish parents. It is always unpleasant to weigh down a young child with additional problems. To make the child Jew-conscious is likely to make him feel different from his non-Jewish schoolchums and playmates; it raises questions in him and may lead to some separation. Is it not better, then, to soft-pedal this problem as long as possible, until the child will be strong enough to "take it"? Should that not be the proper pedagogical policy, at least in all cases where the general environment is sufficiently friendly to exempt the young child from anti-Semitic difficulties?

There is a second feeling often underlying such an attitude among parents. It was typical of a large sector of the Jewish population in pre-Hitler Germany. The projection of the Jewish problem on occasions where there was no absolute necessity was considered dangerous for the Jewish position, as likely to increase the separation of Jews and non-Jews. It was hoped that by, so to speak, de-emphasizing this problem as long as possible and in as many situations as possible the whole Jewish problem would gradually disappear.

It is my belief that such a procedure does not help the child, but, on the contrary, is most likely to have the opposite effect. It is a poor pedagogical policy, likely to bring the child into unnecessarily grave conflicts; it weakens his ability to cope with the difficulties; besides, his behavior is likely not to decrease but to increase anti-Semitism.

To see this clearly we shall have to discuss in some detail the nature of the psychological problems involved from the point of view of the growing child himself.

The underlying problem is not by any means exclusively a Jewish one. A member of any less privileged group has to face it. This holds true to an astonishingly high degree not only in those cases where the lack of privilege comes from social discrimination, as, for example, against Negroes in the United States, but also where it arises from bodily defects, such as deafness. Basic for the general problem is the question: What does belongingness to a group mean to an individual, and how does that affect his behavior in certain situations? I would like to make this point clear by citing two non-Jewish examples.

A young Negress in one of the Northern industrial centers who is engaged in housework is encouraged by her white teacher to take the civil service examination. She passes at the top of the list and is assigned to a public swimming pool. Negro patronage of this swimming pool has been prohibited; nor does the director wish to employ Negroes. His objections are overruled by the civil service authorities. He employs the Negro girl in a lower capacity than she merits—in a cleaning work. The girl works without complaint. After a few weeks she thinks of swimming in the pool herself. Immediately a group of white boys approach her, treat her none too gently, make her stop swimming. The shock is so great that she not only quits her job but refuses to try for any other job to which she is eligible in the civil service. The white teacher from whom I got these facts told me that she came upon the Negress some time later as an elevator girl in a department store. The teacher tried to encourage her to apply once more for a civil service position, but the girl seemed to have lost all faith and all interest in anything better than a subservient place.

Such a degree of breakdown made me suspect that as a child this Negress had had particularly friendly relations with white children on an equal footing. An inquiry showed that she had indeed grown up in a group of children without discrimination between whites and Negroes.

The fundamental problem is revealed even more clearly when we consider certain cases which at first glance seem to have very little connection with minority problems.

The histories of foster-children frequently reveal rather tragic developments. A child adopted very young grows up believing his foster-parents to be his true parents. They do not tell the child the truth, wishing him to regard them as his true parents. It is not unusual, however, that around the age of fifteen or seventeen he is told by someone that he is "merely" a foster-child. The result is frequently devastating beyond all expectation. There are cases where the child who was a good student at school loses his high rank, stops taking any work seriously, and turns into a vagabond. Such reactions have been observed where the foster-parents continue to give the child every proof of their undiminishing love and loyalty, and nothing has changed in the "objective" relations of the family. In such cases the deplorable effect seems to be excessively out of proportion, for nothing else than the child's feeling of belongingness to his foster-parents has been changed.

These experiences have induced authorities in charge of the placement of children to advise the foster-parents to inform the children at a very early age about their true situation. The foster-child is usually told that most children are given to their parents without their choice. He, however, has been chosen by his parents from a whole lot of children and thus can be particularly proud to be a "chosen child." The result often is that the foster-child actually brags that he is a chosen child. In adolescence he has no difficulties in facing problems which might have shaken the very foundation of his existence had he learned the truth only then.

Why does it make so much difference whether the foster-child learns about his true situation at three years of age or at fifteen?

The answer, at least a partial one, can be found in the following fact. The group to which an individual belongs is the ground on which he stands, which gives or denies him social status, gives or denies him security and help. The firmness or weakness of this ground might not be consciously perceived, just as the firmness of the physical ground on which we tread is not always thought of. Dynamically, however, the firmness and clearness of this ground determine what the individual wishes to do, what he can do, and how he will do it. This is true equally of the social ground as of the physical.

The same fact can be stated in another, if more technical, way by saying that the growth of a child to a differentiated and stable person is functionally indentical with, or at least very closely related to, the development of a differentiated and stable psychological surrounding of the child. Seen with the eyes of the child, his world is at first undifferentiated, and only a few areas, such as the experiences of feeding, acquire definite form and color. Gradually the well-defined parts of his world become more extensive. One says "the child learns, acquires knowledge, orients himself." One should realize, however, that this learning means something more: the very building up of the world in which the child lives, of the ground on which he stands. From early childhood, social facts, particularly the feeling of belongingness to certain groups, are among the most fundamental constituents of this growing world, and determine what the individual considers right or wrong, his wishes and his goals.

The behavior of our Negress can now be better understood. She grew up among both Negro and white friends who treated each other on an equal basis. She believed literally in the equal rights which, as she learned at school, are granted by the American constitution to every citizen. In short, she grew up with the ideology of equality of Negro and white, and her outlook for the future was based on this ideology. It is understandable that the world of such a girl must be rocked to its depths the moment this belief proves to be an illusion.

It does not matter very much whether the girl was treated more or less severely in the swimming pool. What matters is the meaning this action had for her change of feeling about the interrelation between the group to which she belonged and the other groups. The girl found the social structure of her psychological world, which had been slowly built up for years, suddenly shattered. Now she was disoriented. She lost the basis for directed actions, for she no longer knew whether an action which she had hitherto believed to be an approach to a certain goal really had that direction. Moreover, her faith in the stability of the world was severly broken down, and without such stability it is meaningless to make plans ahead.

Similar causes are behind the despair of the foster-child despite the fact that "objectively" nothing has changed, and therefore a change of relation to the totality of facts existing in his world has taken place. He too has seen a world built up for years break down in a moment; and his faith in the stability of the ground on which he stands, and hence his willingness to make plans for the future, is lost.

Two deductions for the Jewish problem are easily made:

1. In judging the importance of experiences related to our belonging to, or our status in, a social group, or related to any other constituents of the ground on which we stand, one should not give much weight to the frequency or the unpleasantness of those experiences themselves. Instead one should consider the meaning of those experiences in terms of how much the structure of the life-space of the individual is changed. Jewish parents, then, should learn to realize that it does not much matter how often the child will experience prejudices or whether he will experience them in a rough or polite manner. What is important is the significance of these experiences in determining the position of the Jewish group to the non-Jewish group, and also in determining the sphere of situations in which belonging to the Jewish group is considered a major factor.

2. It is of first importance that a stable social ground be laid very early. The same experience of being called "foster-child" which might upset the fifteen-year-old boy who was not aware of the real situation will have little or no effect at all on the child who was properly introduced to his real situation at the age of three. The variety of social structures to which a growing child can adapt himself in a relatively stable way is astonishingly great. It seems, however, extremely difficult to establish a new stable social ground after one has broken down.

The group a person belongs to serves not only as a source of help and protection; it also implies certain regulations and taboos. In other words, it narrows the individual's "space of free movement." This is very important for the question of adaptation of the individual to the group. The basic problem can be stated thus: Can the individual satisfy his own personal needs to a sufficient degree without interfering unduly with the life and purpose of the group?

If belonging to a certain group hinders rather than helps the individual in achieving his dominant goals, a conflict between him and the group arises, even an eagerness to leave the group. The well known anti-Semitism of some Jews is an expression of the individual Jew's dislike of belonging to the Jewish group. In Germany that was clearly visible in the relation of the German Jews to the Eastern Jews; in the United States in the relation of the Jews of Spanish origin to the Jews of German origin, or more recently of the Jews of German to the Jews of Polish or Russian origin. The same trends exist, clearly enough, between richer and poorer Jewish fraternities on the college campus. There also seems to exist in every underprivilged group a tendency to accept the values of the more privileged group in a given society. The member of the underprivileged group therefore becomes excessively sensitive to everything within his own group that does not conform to those values, because it makes him feel that he belongs to a group whose standards are lower. Such feeling against one's own group conflicts with the natural tendency of the individual in favor of it. The result is a typically ambivalent attitude on the part of members of an underprivileged group toward their own group.

The breakdown of the foster-child has its parallel in the fact that the most severe breakdowns resulting from Nazi anti-Semitism have occurred among half- or quarter-Jews who had believed themselves to be good Catholics or Protestants. These unhappy people experienced a collapse of their social ground when the right of belonging to the group of which they had felt a part all their years was suddenly denied them. Only to a lesser degree, the foundations upon which the Jews have been living all over the world were shaken when Germany, a country considered one of the most enlightened and best educated, restorted to violent Jewish persecution. This was a blow to the favored ideology of many Jews that anti-Semitism should be regarded as a "prejudice" which "well-educated people do not have" and which one can hope to overcome with "enlightenment." It made obvious the fact that this problem cannot be treated on an individual, private basis; it has to be recognized as a social problem of groups.

For the lack of adjustment observable among many Jewish individuals in the United States, another factor might, however, be more important. This is the position of many Jews as "marginal men."

Recently an Eastern college co-ed, keen, beautiful, successful, and therefore on the whole in a particularly desirable position, expressed this feeling as follows:

> "You may have noticed that I am the middle speaker. It's a very appropriate place for me, I think, not because I strike a mean between them, but rather because I am on the fence. I haven't quite made up my mind as to what I think or why I think it. And in that, I am typical of the Jewish people.
>
> "Look at me. I'm neither here nor there. As a Jewess, I don't amount to much. I come to services when I have to; I've been told that mine is a precious heritage, but I haven't the slightest idea what it is. I can name quite a number of relatively unimportant English poets—but do I know who is the greatest Jewish poet? No. My education has been exclusively Christian. My virtues are the Christian virtues—at least my conceptions are. Occasionally, I discover something in me that is characteristically Jewish—and I am surprised, almost estranged from myself. I know I'm Jewish because I've been told so, because I have Jewish friends. Aside from that, it doesn't mean very much to me.
>
> "So you see, as a Jewess I don't amount to much. But I'm not much better as an American either. Here at school I move in a charmed circle of Jews. The other circle, the non-Jews, are oblivious of me, and I of them. Occasionally, the circles touch, sometimes more, sometimes less. I become friendly with someone in the other circle. But self-consciously friendly. If it's a boy, I wonder just how he thinks of me; he wonders what his fraternity brothers are saying. If it's a girl, we both congratulate ourselves mentally on our overstepping the bonds of racial prejudice. When I read the Phi Beta Kappa list, I'm careful to point out how many of the chosen people are Jewish. I'm always conscious that I am Jewish whether I hide it or try to impress it upon others.
>
> "So what am I? According to Jews, I'm American. According to Americans, I'm Jewish. And I'm wrong, utterly wrong, in being that way. And so it is only by pushing people like me off the fence—which side isn't so important, so long as it's off the fence—that Jews are ever going to be freed from anti-Semitism. We must remove the beam from our own eyes."

This uncertainty, which is rather typical of many young Jews, can hardly be due to the fact that the Jewish individual is not only a Jew but an American too. Brandeis's famed statement, in which he urged that double loyalty does not lead to ambiguity, is sociologically sound. That is particularly clear in the United States, which includes so many minority groups of more or less national character, such as the Irish, the Poles, the Germans, the Swedes. Furthermore, every individual belongs to many overlapping groups: to his family, his friends, his professional or business group, and so on. He can be loyal to all of them without being thrown into a constant state of conflict and uncertainty.

Not the belonging to many groups is the cause of the difficulty, but an uncertainty of belongingness.

In practically every underprivileged group a number of people will be found who, although regarded by the privileged majority as not belonging to them, feel themselves not really belonging to the underprivileged minority. Frequently it is the more privileged people within the underprivileged group, or those people whose open or secret intent it is to pass the line, who are in the position of what the sociologists call "marginal men." They are people who belong neither here nor there, standing "between" the groups. The psychological difficulties which the marginal man has to face—his uncertainty, his instability, and often self-hate, due to the more or less permanent state of conflict in which he finds himself—are well known to the student of sociology.

The frequency of "marginal" persons in an underprivileged group is likely to increase the more the differences between the privileged and underprivileged groups decrease, with the resulting paradox that the betterment of the group might increase the uncertainty and tension of the individual.

For the modern Jew there exists an additional factor to increase his uncertainty. He is frequently uncertain about the way he belongs to the Jewish group, and to what degree. Especially since religion has become a less important social matter, it is rather difficult to describe positively the character of the Jewish group as a whole. A religious group with many atheists? A Jewish race with a great diversity of racial qualities among its members? A nation without a state or a territory of its own containing the majority of its people? A group combined by one culture and tradition but actually having in most respects the different values and ideals of the nations in which it lives. There are, I think, few chores more bewildering than that of determining positively the character of the Jewish group. It is not easy to see why such a group should be preserved as a separate unit, why it has not entirely given up its will to live, and why the nations have refused to grant the Jews full assimilation.

No wonder many Jews are uncertain about what it means to belong to the Jewish group, and whether as individuals they should identify themselves with it or should try to break away. No wonder that frequently a Jew may shift his attitude as to what the Jewish group means to him; and if he has lost faith in religion, or has lost belief in what he used to consider the special ideals or mission of the Jews, he is likely to show a strong tendency to break loose from the group altogether.

The position of staying on the boundary between two groups, "on the fence," of being in both groups but really in neither, might be natural for the biological half-Jews. We should realize, however, that a similar and not less difficult situation exists for those who might be called "social half-Jews," those who are not fully decided about their belongingness to the Jews. Those marginal men and women are in somewhat the same position as an adolescent who is no longer a child and certainly does not want to be a child any longer, but who knows at the same time that he is not really accepted as a grown-up. This uncertainty about the ground on which he stands and the group to which he belongs often makes the adolescent loud, restless, at once timid and aggressive, oversensitive and tending to go to extremes, overcritical of others and himself.

The marginal Jew is condemned for his lifetime to remain in a similar situation. Wherever Jewish questions come up he sees with the eyes of both the Jew and the non-Jew. That would be entirely in order if he were clear about the issue and if he knew clearly what his personal values were, because then he would stand on firm ground for making reasonable and fair decisions. The marginal Jew, however, does not as a rule feel sufficiently rooted in either of these groups to be clear and confident about his views and about his personal relations to either side. He is therefore compelled to remain in a rather vague and uncertain but permanent inner conflict. He is the "eternal adolescent." He shows the same unhappiness and lack of adjustment.

History amply shows that "good behavior" on the part of the Jew is by no means an insurance against anti-Semitism. On the whole, the forces acting against the Jews are mainly due to circumstances within the non-Jewish majority, for instance, situations which call for a scapegoat. These forces are to a high degree independent of Jewish conduct. However, as far as the behavior of the Jewish group is relevant, it might well be argued that in the long run more serious trouble develops from their superior than from their inferior performance. In Germany it was the economic, social, and cultural achievement of the Jews that gave momentum to the anti-Semitism. If there were any sense in the attempt to regulate the qualities of Jews, one would have to prescribe this: To avoid anti-Semitism, don't train Jewish youth in superior qualities; make them all average or, even better, below average!

Not altogether happy, either, is the effect which the soft-pedaling and, so to speak, privatizing of the Jewish problem has on the amount of anti-Semitism that the individual Jew is likely to encounter. One might classify Jews into three groups: those who overemphasize their Jewishness, those who behave normally, and those who try to hide or to underemphasize their Jewishness. The individual in the middle group who knows in what situation and to what degree to emphasize his Jewishness probably fares best of all. As for the others, the Gentile is more likely to react without hostility to those who overemphasize than to those who hide their Jewishness. So long as the Gentile does not want a full assimilation, it is evident that he will become easily suspicious against the third type, but feel rather safe in relation to the first. For the uncertain behavior of the individuals in the third group would seem to make it more hazardous to give equal privileges to them than to individuals whose position and probable actions are quite clear: The latter are less likely to use equal status in professional life, business or politics, for the purpose of trying to cross the line in a field where such crossing is not wanted.

SUMMARY

We may conclude, then, that in regard to the Jewish problem the action of Jewish parents should be the same as in matters of sex or any other education, namely, true, open, and realistic. These are the considerations to act on:

1. The basic fact is that their child is going to be a member of a less privileged minority group, and he will have to face this fact.
2. The attempt to keep this problem away from the child as long as possible, and soft-pedal it, will in all likelihood make for greater difficulties in adjustment later on.
3. This applies just as much in a community where the child is so fortunate as not to encounter anti-Semitic difficulties in his early life; parents should realize that the problem is bound to arise at some time, and the sooner it is faced, the better.
4. Such an early build-up of a clear and positive feeling of belongingness to the Jewish group is one of the few effective things that Jewish parents can do for the later happiness of their children. In this way parents can minimize the ambiguity and the tension inherent in the situation of the Jewish minority group, and thus counteract various forms of maladjustment resulting therefrom.

5. Outstanding among the techniques that parents should employ is the treatment of Jewish problems not as an individual and private matter but as a social issue. For instance, to press the child harder for good behavior, or to raise his personal ambition higher than is customary in the Gentile majority, puts the child merely in a state of keener tension that makes for less easy adaptation. Parents should from the very beginning stress the social aspect of the situation. This is more realistic and helps to prevent the personal uncertainty and self-accusation or self-pity which otherwise are the likely results of anti-Semitic experiences.

6. A better understanding of the sociological problems involved is of particular value to the Jewish adolescent. For it can help him to solve one of the most bewildering puzzles, mentioned previously —what kind of a group the Jews are, whether he personally belongs to them. He will often feel himself more like some Gentile friends than like some Jews, and he is apt to make this feeling of similarity or dissimilarity his measuring-stick for group-belongingness. It is true that some sociologists have made one or another kind of similarity between members the defining mark of a group. However, similarity between persons merely permits their classification, their subsumption under the same abstract concept, whereas belonging to the same social group means concrete, dynamic interrelation between persons. A husband, a wife, and a baby are less similar to each other, in spite of their being a strong natural group, than the baby is to other babies, or the husband to other men, or the wife to other women. Strong and well-organized groups, far from being fully homogeneous, are bound to contain a variety of different groups, but social interaction or other types of interdependence. A group is best defined as a dynamic whole based on interdependence rather than on similarity.

As a rule, the adolescent is well able to understand this fact. It will help him to see that his belonging or not belonging to the Jewish group is not a matter mainly of similarity or dissimilarity, nor even one of like or dislike. He will understand that regardless of whether the Jewish group is a racial, religious, national, or cultural one, the fact that it is classified by the majority as a distinct group is what counts. He will be ready to accept the variety of opinions and beliefs or other dissimilarities within the Jewish group as something quite as natural as in any other group. He will see that the main criterion of belongingness is interdependence of fate. Young American Jews may abhor Jewish national mysticism; they may not be willing to suffer for cultural or religious values which they do not fully understand, or perhaps even dislike; but they must be sufficiently factminded to see clearly their interdependence of fate with the rest of the American Jews and indeed with the Jews all over the world.

The feeling of belongingness based on such a realistic sociological understanding of interdependence would, I think, go a long way toward a proper balance in Jewish action. It should prevent the individual from overemphasizing the Jewish problem, but at the same time create a willingness to accept a fair amount of responsibility for his own group. It should help to clear away the fog of uncertainty and conflicting feelings, which are paralyzing the action of so many of us today.

7. Parents should not be afraid of so-called "double allegiance." Belonging to more than one overlapping group is natural and necessary for everyone. The real danger lies in standing "nowhere"—in being a "marginal man," an "eternal adolescent."

Six—The Psychological Ecology of a Small Town

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THE EDITOR GRATEFULLY ACKNOWLEDGES THE KINDNESS OF THE AUTHORS IN PERMITTING THE INCLUSION OF THIS REPORT, WHICH HAS NOT PREVIOUSLY APPEARED IN PRINT. IT IS ADAPTED FROM A PAPER PRESENTED AT THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE IN DECEMBER, 1950.

As indicated by the authors, most studies of child behavior have been studies of the child in a stabilimeter, in a nursery school, or in an experimental situation; few of them have been studies of the child in his home or in his community. Such studies are difficult to conduct and for the most part, we lack methods for making such investigations. Barker and his associates, as described below, are doing pioneer research in this area. The following report gives only a sample of the methods that they are employing and of the problems that they are attacking.

It is an interesting fact that scientific psychology has been predominantly an experimental science from its beginning. The first scientific psychologists were experimenters who worked in laboratories; even clinical and industrial psychologists have been experimentalists arranging and varying conditions in order to test hypotheses and hunches. The descriptive, natural history, ecological phase of science, so prominent in the biological sciences, in sociology, in anthropology, in the earth sciences and astronomy, has had very little representation in psychology. This has left a serious gap in psychological knowledge. Experimental procedures have yielded some understanding of the laws of behavior. We know how people behave under the conditions of the experiments and clinical procedures that have explored these laws. But virtually nothing is known about the occurrence of behavior and psychological conditions outside the laboratory and clinic. We know how men *can* behave but not how they *do* behave. For information about the actual behavior and life situations of people we must go at the present time to novelists, biographers, diarists and journalists.

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[1] This paper has been prepared by the authors named, but it represents the thinking of the whole staff of the Midwest Social Psychology Field Station. The research reported has been supported by a research grant from the National Institute of Public Health, U. S. Public Health Service, and by the University of Kansas. We are greatly indebted to the people of the town of Midwest for their cooperation.

It is different in other sciences. Geologists, biologists, chemists and physicists can tell in detail about the distribution in nature of the materials and processes with which they deal. But no psychologist knows more than laymen about the sources, the frequency, the degree and the behavioral effects of frustration and freedom, of reward and punishment, of conflict, of hostility, of social pressure, of success and failure in the lives of men. The result is that psychologists can do no more than speculate at the present time on many important questions. What changes have occurred over the generations in the way children are reared and in the way they behave? How does life differ for children in large and small families? Are American children disciplined differently from English and French children? If so, does this affect the national character of Americans, Englishmen, and Frenchmen? How, in psychological terms, does life differ for rural, town, and urban families? No accurate psychological data bearing on these questions are available and they cannot be secured in laboratories.

The present lack of ecological field data limits progress in psychology on more theoretical levels, too. It is impossible to create in the laboratory the frequency, the duration, the scope, the complexity and the magnitude of some conditions that are widespread in nature. In this, psychology has much in common with meteorology. Some of the principles of the whirlwind and the thunderbolt can be studied in the laboratory, but to extend the curves into the high values it is necessary to get out to the plains of Kansas. In principle, the same is true in psychology for studies of frustration, conflict, social pressure, and a hundred other conditions that are frequent in daily life but difficult to create experimentally in extreme forms or for long periods of time. This should not be discouraging. Psychological experiments in nature are occurring every day and we need only the proper techniques and facilities to take advantage of them.

Public health scientists have long used naturally occurring differences in sanitation, population characteristics, and nutrition to test hypotheses about the etiology of health and disease. Astronomers, agronomists, geologists, paleontologists and anthropologists regularly test basic scientific theories in naturally occurring situations. When the great theoretical advances of these so-called descriptive sciences are considered, it appears reasonable to hope that similar progress may be made in psychology if adequate field methods are developed.

Adequate ecological studies in psychology must record naturally occurring situations and behavior without appreciably disturbing either. We have tried to develop methods for doing this in studying the children of Midwest, a small town in the central part of the United States. Our experimentation with a variety of observational and recording techniques indicates that the only satisfactory method is one which the citizens of Midwest themselves use regularly, namely, the one of looking, listening, and telling. We have developed this method in the three directions of *the behavior settings survey*, *the specimen record*, and *the instrument for behavior analysis*. Each of these will be considered in turn, and we will present some sample results.

THE BEHAVIOR SETTINGS SURVEY

The town of Midwest has a vast number of physical and social parts. A crew of physical and social scientists could make a limitless list of its characteristics. Here are some of them.

Population	730
Elevation	780 ft.
Area	300 acres
Number of dwellings	269
Number of grocery stores	6
Mean temperature	54 degrees
Races	40 Negro, 690 white
Industries	Distribution Repair Personal service Government
Annual Precipitation	34 inches
Number of children under 12 years of age	119

This list could be extended indefinitely; it would constitute a catalogue of the physical and social milieu of the community.

The people of the town discriminate only a few of its parts. Although the number of varieties of trees a botanist could identify within the limits of Midwest probably would reach a score or more, the citizens generally recognize only six or eight kinds. The town of Midwest is seen as a 300-acre mosaic with thousands rather than millions of separate pieces and these are formed into a much more limited number of perceptual units. *Behavior settings* make up one class of such perceived units; they are parts of the physical-social milieu that are seen by the people of the community generally as appropriate for particular kinds of behavior. Not all parts of the milieu are behavior settings because all are not perceived as having differential behavior possibilities or requirements. Yellow and blue objects are discriminated, but they are not necessarily seen as requiring different kinds of behavior. On the other hand, houses and stores, which are also discriminated parts of the milieu, *are* seen as requiring different kinds of behavior. When a man visits a store, he takes money with him and he keeps on his hat; when he visits a house, money is not required and he removes his hat. Behavior settings, then, are perceived units of the community mosaic which, in addition to having other properties, are seen as having "behavior appropriateness" or "behavior requiredness."

The sources of the behavior appropriateness or requiredness of behavior settings is a very interesting matter that cannot be considered here. Physical properties and arrangements, social pressures, native reactions and learned responses all contribute. For the present purpose, it is enough to say that the community is seen by its people as having these parts and that this differential perception can be easily observed by an investigator. The behavior settings of Midwest can be observed as easily as the sex, age, and race of the residents; and they appear to have as coercive an influence upon behavior as these other more familiar correlates of behavior uniformities.

The behavior settings of Midwest are limited; there were 453 *unit settings* in the town in 1949. A unit setting is a complete, perceived unit, one which is not a part of any other behavior setting. The Jones family and the Presbyterian Church, for example, are not seen to be parts of each other or parts of any of the other 451 unit settings. Unit settings differ in importance for different individuals and classes of people. One measure of their importance is their temporal weight, i.e., the amount of time people spend in them. Temporal weights have been computed for the children of Midwest under 12 years of age. Our data indicate that the unit setting, *school,* has a weight of 450; this means that an average of 450 hours per year is spent in the school setting by each child under 12. Thus, the temporal weight of a behavior setting is defined as the number of hours per year per child spent in that setting. The ninety-five unit settings with weights of 10 or greater are listed below. These weights are approximate. They are based upon records of attendance, when these were available, and upon reports of informants, casual observations, and specimen day records, which will be described later.

Behavior Setting	*Temporal Weight*
School	450
The Culver Family	215
The Hope Family	130
The Matson Family	120
Moving Picture Theatre	70
Sidewalks and Streets	70
Families #4-#25	60-75
Families #26-#53	26-50
Methodist Church	30
Presbyterian Church	28
Courthouse Lawn	20
Christmas	18
Families #54-#77	16-25
Brownie Scouts	12
Cub Scouts	12
Clifford's Drug Store	12
Jones's Grocery Store	12
Smith's Grocery Store	12
Department Store	10
Fourth-of-July	10
Thanksgiving	10
Halloween	10
Carnival	10
Variety Store	10

Some unit settings are similar or almost identical in behavioral significance. As unit settings, for example, different families have roughly the same behavior appropriateness for those who enter them. The same is true of lumber yards or barber shops or lawyers' offices. Accordingly, identity groups or broader equivalence groups of settings can be formed. Major equivalence groups with the frequency and total temporal weight of the settings in each, are shown below. Each setting enters this tabulation once. The categories are commonly used by the people of Midwest.

Equivalence Group	*Frequency*	*Temporal Weights*
Family settings	263	3400
School settings	6	460
Business settings	92	230
Social and recreational settings	55	111
Church settings	13	65
Government settings	14	10
Service settings (e.g., Red Cross)	10	6

Behavior settings can be classified according to the functions which they are seen to serve in the community. Some have an educational function; others serve such functions as recreation, health, or religion. Most settings are seen as having more than a single function; the setting, *Christmas Day*, for example, serves religion, recreation, social contacts and nutrition. The frequencies and weights, for children, of non-family, unit settings are given below according to function. Most of the different settings enter this tabulation several times.

Function	*Frequency*	*Temporal Weight*
Recreation	67	825
Social interaction	83	787
Aesthetic experience	26	708
Nutrition	49	673
Education	34	602
Health	24	531
Business	95	262
Service (philanthropy)	28	147
Religion	20	138
Shelter	26	113
Transportation	12	93

A unit setting may, and usually does, include *subordinate settings.* The unit setting, *Family,* for example may include *Breakfast, Bedtime,* and *Chores.* All such minor subdivisions of the community mosaic can be analyzed in the same way as unit settings.

The foregoing may be enough to indicate the usefulness of the concept of behavior setting for the purpose of describing the psychologically relevant parts of a community. Behavior settings, it is emphasized, are not psychological situations; they are not defined in terms of unique meanings for particular persons, but in terms of common or average perceptions. The concept appears nonetheless to provide a useful means of approach to the psychological conditions of life which a community imposes upon its citizens. Comparative studies of the behavior settings of different communities seem promising. Studies of changes in settings within the same community also should be of significance; the catalogue of behavior settings in Midwest changes week by week. This appears to be one of the processes by which the community adapts and regenerates itself.

THE SPECIMEN RECORD

The *specimen record* is a detailed, narrative account of a long segment of a person's behavior and situation as seen by skilled observers. In its most highly developed form it covers a day in the life of a child. Such a record is made by a team of 7 to 9 observers who take turns throughout the day. It describes in concrete detail the stream of a particular child's behavior and psychological habitat; the record usually covers over 300 typed pages. Shorter specimen records are made in particular behavior settings.

At its best, this kind of record preserves some of the continuity and complexity of the behavior and some of the content and meaning of the situation. Here is a sample from a day record on Roy Eddy.

On the day of the record, May 12, 1949, Roy was seven years old. He lived in Midwest with his mother and father, two sisters (15 years and 13 years) and brother (12 years). The following sequences took place after Roy had finished breakfast and before he left for school. Roy was dressing in his bedroom, which also serves as a hallway between the kitchen and the living room. He stood in his shorts beside his bed.

8:33 A.M.—Roy took a pair of neatly pressed jeans off the bed and viewed them soberly.

He sat on the bed as he put his legs into the legs of the jeans. He was puffing with the effort of dressing, and probably because he had a cold, too.

One of the snaps on the fly wouldn't snap, though he tried hard, frowning impatiently.

He said, "Ounnn," in protest and looked at me seriously for me to share this reaction to difficulty.

I said, "It's hard." He said, "Urrrr," softly in agreement.

With another stronger effort he succeeded in fastening the snap.

> He showed no sign of satisfaction, however, as his attention quickly and eagerly turned to a belt on the bed. The belt seemed to be the important thing, but the jeans had had to go on first.

He straightened the belt, but was not ready to put it on.

8:34 A.M.—He announced, "I'm going to take my belt," as though this were something very important.

Then he put on the belt. This completed the dressing.

Roy said proudly and with definiteness, "I'm going to take my gun, too."

He looked at me and said, "The gun is still under my pillow."

He then looked toward his pillow.

> His tones and glance suggested that there was prestige about sleeping with a gun under the pillow. However, there was no arrogance, just happiness.

He reached under the pillow.

To his evident surprise, he brought out a flashlight.

He said, "Ahhh, here's the flashlight. I didn't put it up yet." He laughed as though he had been quite inefficient, that he should have done that some time before.

He took the flashlight, and laid it carefully on a ledge of the nearby cupboard.

8:35 A.M.—Lola, his fifteen-year-old sister, called pleasantly, but with responsibility, "Are you about ready to go to school, honey?"

As Lola came in, Roy got his gun and holster from under the pillow. Lola went directly to Roy and turned his collar down.

She said, asserting her responsibility, but in a friendly voice, "Collars look better when they are turned down."

She kneeled and turned up Roy's jean legs so they wouldn't be too long.

She was quite motherly about this. Roy was absorbed with his gun and holster, paying little attention to his sister.

Then Lola noticed Roy's holster and gun and said, "You shouldn't wear that holster and the gun," in an authoritative tone.

He ignored her remark and quietly persisted in preparing to wear the gun.

He was getting the holster placed just right on the belt.

Then he took off a chain that was on the holster.

He put the chain on the bed.

His sister said, "You can't wear it." Then as a more forceful approach, "Mother doesn't want you to wear it." She said this firmly and with agitation.

Roy showed no intention of heeding her. He adjusted the holster even more; then put the belt around him and began to fasten it.

8:36 A.M.—His sister exclaimed quite loudly as she saw him actually put on the belt, "Roy, you take it off!"

Roy stood resolute and very quiet.

> This did not appear to be an ominous quietness perhaps to be followed by tears or tantrum. I think Roy had decided he didn't necessarily win by argument or pleading a cause, but that it was more prudent to act quietly, avoiding interference. It was a technique, it seemed, for coping with a sister who would try to prevent him from doing what he wanted to do. Lola did not show hostility during the argument. She meant to be firm because she was responsible for his dressing, but she did not show much agitation until she saw him actually putting the belt on.

She seemed finally to be almost ready to concede the point, and said, "Do you play guns at recess?"

He said, "Yes," emphatically and also defensively, looking straight ahead.

Vernon, (the twelve-year-old brother) who came in to hear the argument, said, knowingly and in firm contradiction, "Oh, no you don't." Lola said uncertainly and still trying to reckon with Roy, "Well, will it be warm enough today?"

I took it the argument was over and Roy had won.

The sister said, "I'll wash you." She started for the bathroom, expecting Roy to follow.

We look upon specimen records as museum materials to be used for many purposes. A partial account of how we have used them follows.

THE INSTRUMENT FOR BEHAVIOR ANALYSIS

In order to raise our descriptions of psychological habitat and behavior above the level of good reporting, it has been necessary to develop a method of conceptualizing and quantifying the raw data in specimen records. The instrument for behavior analysis does this in two steps. First, it establishes criteria for dividing a record into meaningful units of behavior, called episodes. Second, it provides over 150 categories for describing the behavior and situtaion of each episode.

Here is an episode from a specimen day record on seven-year-old Raymond Birch. Raymond was dressing in his own room in the morning.

7:06 A.M.—The pet dog, Honey, ambled into the room. Honey was a fox terrier, quite old, fat, and broad.

While buckling his belt Raymond said, with a little sleep in his voice, but friendly and pleasantly, "Hi Honey."

Honey put her front paws on Raymond's knees.

He finished buckling his belt and, at the same time, patted Honey and scratched her back in a friendly way.

The following is an episode from a record on Dale Green who was four years old. Dale was playing in the living room of his home in the evening.

Spot, Dale's Collie dog suddenly began to bark.

8:54 A.M.—Dale made a beeline for the back door saying, "No, Spot, no; shut up, goddam you."

He repeated this again and again, louder and louder until he was simply screaming it.

He must have said it eight or ten times.

It was Mr. Green coming home in his truck that made Spot bark.

As soon as Dale saw his father's truck, he stopped yelling.

We take every such episode in a specimen record, much as a histologist takes a slide or a medical technician takes a sample of blood, and ask a number of questions about it via the instrument. The following are sample questions and answers about the psychological habitats and the behavior of Raymond and Dale in the episodes, *petting Honey* and *trying to silence Spot.*

General content of habitat?	For Raymond (R): Social; not physical, intellectual or autistic For Dale (D): The same

Behavior objects in habitat?	R: Pet dog D: Pet dog
Meaning of dog's behavior?	R: Affiliation, deference, succorance D: Autonomy, contrariance, exhibition
Need centrality?	R: Could not judge D: Medium centrality
Need strength?	R: Could not judge D: Medium strength
Valence: sign?	R: Positive D: Positive
Valence: strength?	R: Medium strength D: High strength
Geniality of habitat?	R: Beneficient D: Inhospitable
Motivational structure of habitat?	R: Consummatory; no task, path, rewards, or punishments present D: Means-end task
Frustration: strength?	R: Zero D: Medium
Conflict: degree?	R: Zero D: Zero
Freedom: degree?	R: Great freedom D: Small freedom
Cognitive field: clarity?	R: High clarity D: Medium clarity
Cognitive field: size?	R: Small size D: Medium size
Time perspective?	R: Present only D: Present and future
Aspiration level?	R: None; no task or problem with respect to which a level of aspiration could be set D: Medium high aspiration
Degree of overlap of habitat?	R: One overlap. "Petting Honey" overlaps with "getting dressed" D: One overlap. "Attempting to silence Spot" overlaps with "Discovering who is coming"

Compatibility of overlapping habitats?	R: "Getting dressed" impedes, but does not seriously interfere with "petting Honey" D: No interference
Reality-phantasy level?	R: Reality completely D: Reality completely
Direction of behavior?	R: Consummation D: Approach
Modality of behavior?	R: Affiliation, nurturance D: Dominance, aggression, counteraction
Behavior actones?	R: Motor, perceptual, verbal D: Motor, perceptual, verbal
Intensity of behavior?	R: Low intensity D: High intensity
Efficiency of behavior?	R: High efficiency D: High efficiency
Creativity of behavior?	R: Low creativity D: Medium creativity
Maturity of behavior?	R: Appropriate for both child and adult D: Appropriate for adult
Behavior affect?	R: Medium satisfaction D: Medium dissatisfaction
Emotionality of behavior?	R: Expansive, happy D: Tense, excited
Humor?	R: Not present D: Not present
Play?	R: Not present D: Not present
Issue?	R: Does not apply; no difficulty D: Non-attainment

A number of technical problems concerned with the accuracy of episode ratings remain to be solved. The work done thus far however, has demonstrated the possibility of analyzing a single episode from a long record of behavior and situation in terms of a list of descriptive categories—if not the present list, then others that are technically more adequate—and in this way defining the psychological habitat and behavior of an individual.

We have under analysis at the present time several thousand episodes of children's behavior from all the behavior settings of Midwest. We believe that the results will make it possible to describe in conceptual and quantitative terms the psychological living conditions and behavior of the children of Midwest. A complete description of this kind could later be used as a base for comparative studies in other communities and in the Midwest of later years.

SAMPLE RESULTS

Findings from applications of the instrument for behavior analysis may be illustrated by means of data from two day records. These records and the analysis that we have made of them provide, in a way, a test of our descriptive methods, for there is reason to believe that the psychological habitats of the subjects, a boy of Midwest and a boy of a neighboring town, differ markedly. At the time the records were made, one of the boys, Raymond, was, in the words of the people of Midwest, a "real boy," "a good, average seven-year-old." Bobby, on the other hand, was looked upon by all as a tragic case. A congenital heart defect had required him to live his 7 years of life under very unusual circumstances. The blue cast of his skin marked him off immediately as a freak. His parents and friends, knowing that, short of a miracle, he could not live more than 8 or 10 years had very special, emotional relations with him. His great sensitivity to fatigue and infections required that they protect him from normal social and physical activities. His exhaustion after walking at a normal rate for fifty feet or running six steps, and the numbness, stiffness and weakness of his hands limited his behavior greatly.

It would appear that Bobby must live in a psychological world differing in important ways from Raymond's. Both laymen and scientists probably would assume as much; but the actual psychological conditions imposed by a physique like Bobby's have never been specified. Let us see what the analyses of the records on Raymond and Bobby reveal as to the differences between them in habitat and behavior. Here are some data yielded by the instrument for behavior analysis.

Episodes

	Raymond	*Bobby*
Number of episodes	1014	708
Number of analyzed episodes*	712	496
Habitat Findings		
Percent of episodes in which *habitat* was rated as:		
Beneficient	70	83
Providing gratuities or help	3	16
Lacking barriers	37	60

* Minor, overlapping episodes were excluded from the analyses. The duration of the waking day was, for Raymond, 13½ hours and, for Bobby 13 hours.

Habitat Findings—continued

	Raymond	*Bobby*
Involving present only	27	41
Involving present and future	60	36
Of high cognitive clarity	62	39
Providing an appreciative associate	7	15
Providing a deferent associate	3	12
Providing a nurturant associate	5	11
Providing a warm, friendly associate	31	57
Providing a dominant associate	21	14
Frustrating	7	6
Large size	65	66

Behavior Findings

Percent of episodes in which *behavior* was rated as:		
Predominantly large muscle	43	6
Predominantly verbal	22	44
Leading to success or achievement	78	76
Leading to failure or loss	7	9
Involving emotional actions	38	50
Unhappy actions	3	7
Happy actions	21	31
Involving social pressure upon another person	12	25
Involving aggression toward associate	3	6
Involving dominance toward associate	4	10
Involving exposition toward associate	5	12
Showing avoidance of blame	7	2
Showing avoidance of harm	3	0

It will be noted that Raymond's habitat was a beneficent one, but that it provided him with few outright gifts and confronted him with many barriers. Although Raymond's associates were frequently warm and friendly, they did not often explicitly express appreciation or deference, and they not infrequently dominated him. In this situation Raymond was quite often happy and infrequently aggressive or dominating. He was frequently successful or achieved what he set out to do.

Bobby's habitat was the more beneficent; it conferred more gratuities and presented fewer barriers. Bobby's associates were more often friendly, appreciative and deferent. He lived more often in the present and less often in the future than Raymond. His world was less clearly structured. Frustration was no more frequent for Bobby than for Raymond. Bobby was much more verbal than Raymond; he was more emotional, more aggressive and more dominant with his associates. Bobby and Raymond both failed and succeeded about an equal number of times.

Inferences about the psychological habitat of a person can sometimes be drawn from information about the behavior settings which he enters. The behavior settings entered by Raymond and Bobby with the time spent in each, were as follows:

	Raymond	*Bobby*
Number of behavior settings	28	19
Duration of behavior settings (in minutes)		
Getting-up-time	12	10
Bathroom	13	5
Breakfast	10	14
Indoors, free	50	434
Own yard, free	73	55
Lunch	19	16
Supper	19	16
Bed time	10	26
Sidewalk	20	15
Park	0	24
Vacant lot	74	0
Courthouse lawn	111	0
Resting time	0	11
Schoolwork at home	0	63
Chores	0	33
Bus	0	5
Private car	0	6
Grocery store	0	17
Cafe	0	23
Courthouse	38	0
Neighbors yard	6	0
Music practice	7	0
School, study and lessons (8 settings)	236	0
School, play and free time (5 settings)	87	0
All intra-family settings	213	683

From these data it is clear that Bobby's psychological habitat was formed within a framework of fewer and less varied settings than Raymond's. He had to play his part in a drama with fewer, more homogeneous, and more familiar scenes and properties. This means that Bobby's world was potentially less stimulating and actually less demanding in terms of the behavior adjustments required. The smaller scope of the behavior settings within which Bobby's psychological habitat was formed seems to be largely due to direct and indirect effects of his physical limitations, i.e., to his locomotor and manipulatory difficulties, and to the social restraints and pressures placed upon him by his associates.

CONCLUDING STATEMENT

The data reported here may give an idea of methods which have proven useful in studying the psychological ecology of the children of Midwest. Other methods await development. It appears to us that if psychology is to provide the guidance of human affairs that is so widely hoped for, it must match its experimental and clinical procedures with field methods that will make it possible to assess existing psychological conditions and behavior.

Our preliminary data suggest that the town of Midwest is a good place for young children. Children have great freedom in Midwest; their range of experiences is broad; they live close to some of the fundamental realities of life; they are people of importance. We need quantitative indices that will allow us to compare Midwest with other communities in these terms, and ultimately to study outcomes for mental health and illness. Public health physicians, meteorologists, and soil chemists can measure the sanitation, the weather and the soil fertility of a region with precision. As yet psychologists have only the crudest indicators of the psychological climate that prevails. Here lies a fundamental task for a psychological ecology.

X–THE CHILD IN SCHOOL

One—Children's Social Adjustment in Nursery School

ARTHUR T. JERSILD & MARY D. FITE

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This study is a model of caution and thoroughness in the treatment of data. It will be apparent to the student that a superficial consideration of the results would have led to conclusions quite different from those arrived at by the authors.

This study deals with the effect of nursery school experience on children's social behavior and social adjustments. One of the main claims that has been made for nursery school education is that it promotes the child's social development. Several studies, some of them somewhat informal, have tended to corroborate this claim. In a recent quite extensive study by Hattwick (4), the quantitative findings indicate that the children who had attended nursery school for some time, as compared with children who had just entered, showed less fear of strange people, less tendency to shrink from notice, less tendency to stay close to adults or to avoid play with other children. These influences, according to Hattwick (4), seem to occur "over and above the improvement which would naturally come with age."

Other studies have shown somewhat similar trends. The effects that have been noted however, have not all taken the form of increased amiability and cooperation. In a study by Caille (1), it was found that there was a positive correlation between resistance to other children and length of stay in the nursery school. A study by Ezekiel (2), suggests that there is an increase in egocentricity—defined as "attempts to make onself the center of an activity"—with stay in the nursery school.

In a study by Jersild and Markey (6), it was found that there was an increase in the number of children's fights and quarrels with nursery school attendance. This increase in conflicts did not, however, seem to be due primarily to an increase in sheer combativeness; it came more as a by-product of an increase in the children's activities and social contacts.

On the one hand, studies of the effect of nursery school experience have shown an increase in social participation and a decrease in the solitary and onlooker forms of behavior. On the other hand, the findings also show an increase in independence of others and an increase in self-assertion. In other words, as far as quantitative findings go at present—and they are admittedly rather meager—nursery schools tend to accomplish the rare achievement of promoting the child's sociability and at the same time fostering his individuality and independence in a social group.

The present study was designed as a further exploration of the effects of nursery school attendance. The inquiry had a twofold purpose: First, to discover such quantitive trends as might emerge from the study of the limited number of children included in the investigation; second, and more important, to go behind the averages and correlations to appraise the adjustments of individual children. The latter line of study followed, in some respects, the fruitful lead of Murphy's study of sympathy in early childhood (8), (9), and of a study by Fite, dealing with preschool children's aggressiveness, attitudes toward aggressiveness, and children's verbalizations regarding rules of conduct as compared with their actual behavior.

SUBJECTS

The subjects included 18 children, representing all the regularly enrolled members of a nursery school group. In addition, records were obtained of the behavior of a number of other children who were irregular in attendance or who were members of an older group that used a part of the playground occupied by the subjects of this study.

At the beginning of the investigation, the subjects ranged in age from two years, ten months, to three years, eleven months. The average age was three years, five months.

The 18 subjects included 9 children who, during the preceding year, had been members of nursery school groups housed in the same building. All of these 9 had old friends and acquaintances among the children in the study. The subjects also included 9 children who were entirely new to each other and to the group. Seven of these had never previously attended nursery school; the remaining two had attended separate nursery schools the year before, but were strangers to each other and all the rest.

PROCEDURE

The main procedure in the study was direct observation. In the fall, as early as possible, each of the 18 children was observed during ten 15-minute intervals, representing two and one-half hours devoted to each child, and a total of 45 hours of systematic observation of the group as a whole, apart from further periods of informal observation. In addition, use was made of case records of individual children, teachers' reports, and other data that were available from other studies of the same children.

In the spring, 2 of the original 18 children were not available. Each of the 16 who remained was observed during 8 five-minute periods.

The observational records were made in the form of a running, diary account, similar to the procedure followed in earlier studies by Jersild and Markey (6), (7).

RESULTS

One form of treatment was to note and tally the children's social contacts. For each child a score was computed, representing the number of 30-second intervals during which he entered in a social contact with one or more other children. A "social contact," as treated in this study, represents any occasion in which, to all appearances, there was an interchange between one child and another. This includes all cooperative, organized play, sharing of materials or activity, physical contacts, and conversation. It does not include "onlooker" activity, watching, or parallel activity unless an actual interchange had taken place.

The results from this treatment were quite interesting. In the fall, the intervals of social contacts exhibited by children who had had previous nursery school experience and who had old acquaintances in the present group far exceeded those exhibited by other children. The former children will be referred to as the "old" group and the latter, the "new" group—meaning old or new not in age but from the point of view of previous nursery school experience and previous contact with other children in the group.

In the fall, the maximum obtainable score was 300. The scores of the 18 children ranged from 18 to 220, with an average of 111.1, or 37 per cent of time spent in social contact, according to the definition above.

"Old" children showed an average of 144 intervals of contact, or 48 per cent. "New" children (including the two who had attended schools but were strangers to all the present subjects) had an average of 78, or 26 per cent. The "old" children exceeded the "new" almost two to one.

If the two children who were new to the group, but who had previously attended nursery school, are counted with the "old" rather than the "new," the difference is still greater, with a score of 64 for "new" children and 141 for the "old."

Here is rather a striking difference, which seems to speak well for the effect of previous nursery school experience on children's ability to make contacts and to fraternize freely with others.

However, when matters are examined more closely, both from a quantitative point of view and from the point of view of individual children, it appears that the difference is somewhat deceptive. For, when the results are treated in smaller sections (which, of course, means that the reliability is lowered), quite a different picture emerges.

Even during the first few weeks of school, the "new" children were making rapid gains on the "old." During the interval covered by the first half of the observation in the fall, "new" children made only 54 per cent as many social contacts, on the average, as did the "old."

But, during the second half of the observations—still in the fall, and still not many weeks after the beginning of school—"new" children had progressed to the extent that they made 80 per cent as many contacts, on the average, per unit of time, as did the "old." In other words, within a few weeks, the "new" children had made up most of the advantage, if such it can be called, which the "old" children had apparently acquired from previous nursery school attendance during one or two years.

These trends appear still more strongly in the spring. The spring records show an increase in number of social contacts per unit of time as compared with the fall, when the records of all children are considered together. Fifty-eight per cent of the time was spent in social contact in the spring, as compared with 35 per cent in the fall.

This gain was shared by both the "old" and the "new" children, but the latter gained much more than did the former. The "old" children increased from 48 per cent to 58 per cent. The "new" children increased from 26 per cent to almost 59 per cent. If the two children who were strangers but who had gone to other nursery schools are treated as "old" rather than "new" subjects, the "new" group still scores practically as high in the spring as does the "old." The respective percentages are 61 for the "old" and 55 for the "new."

As far as group trends are concerned, in the matter of social contacts, it appears, then, that the "new" children had progressed as much during a year in nursery school as had the "old" children during the same school year plus one or two preceding years of nursery school attendance. These averages would look more impressive if the numbers were larger. Even if the number were larger, however, and the averages thoroughly reliable, they still would tell only a small part of the story, according to further examination of the results. Almost every department of this study emphasizes how deceptive averages may be when they are examined in the light of closer study of the individual scores included in the averages.

When we look at the records of individual children in this as well as other divisions of the study, it appears that the scores obtained by simple counting of social contacts may mean quite different things in the case of different children.

Taken at face value, the averages indicate that the "old" children were at the start more sociable than the "new." In many individual instances, however, this was not found to be entirely the case. The records show that the larger scores made by the "old" children were not due so much to a general increase in sociability—expressing itself in more ubiquitous contacts with other members of the group—so much as it was due to a carryover of special ties and friendships from the year before.

For example, the child who stood highest in the fall, with a score of 202, made 132 or 65 per cent of these 202 intervals of contact with one other child, a friend during the preceding year in nursery school. The next highest child made 192 intervals of social contact in the fall records; 108 of these consisted of contacts with one single companion from the previous year. In the case of the child who stood third from the highest in the fall, 42 per cent of all intervals of contact were limited to one child, and the child making the fourth from the highest score restricted 48 per cent of her contacts to one child who was her special companion.

In addition, apart from contacts limited to a single friend carried over from the preceding year, a large number of the remaining contacts were restricted to old acquaintances from the preceding year. In the case of one of the "old" children, who made a high score of 175 intervals of contact during the fall records, only eight contacts were shared with children whom the child had not known the year before.

In other words, much of what seems at first glance to be an increase in sociability derived from nursery school attendance does not represent so much a general increase in ability and inclination to fraternize freely with others as it represents a carryover of special companionships.

This does not mean, however, that *all* of the apparent initial advantage of the experienced children was due to close ties with individual friends. Even when these close companionships are discounted, the evidence as a whole still indicates that the "old" children would somewhat exceed the "new" in social contacts during the first few weeks of school, although not nearly to the extent that the averages at first glance would suggest. The fact that previous experience in nursery school may promote a child's ability to make social contacts, even though no previous acquaintances are present, it suggested by the behavior of the two children who were new to the present situation and strange to all the children, but who had attended other nursery schools the year before. Both of these children made very low scores during the first part of the period of observation in the fall, but thereafter they gained much more rapidly than did children who not only were new to the present group and situation, but who also had never attended nursery school before. (One child increased his social contacts per 15 minutes from 6 in the first 5 records to 24 in the second 5; the other had an average of 6.1 during the first 7 periods and an average of 21.3 during the remaining 3.

The present abbreviated report will offer a few illustrative items rather than a review of all the other various phases of the study, dealing with group trends and individual characteristics in such matters as conflicts, leadership, imaginative behavior, resourcefulness, and the manner in which individual children responded to the nursery school environment.

It has already been implied that averages and other statistical values may be quite deceptive and conceal more than they reveal. This is especially true if we follow the hallowed procedure of noting simply the occurrence or nonoccurrence of a phenomenon without regard for the context in which the phenomenon occurs.

Two children might make almost similar scores on a given tabulation, and yet the two scores might mean quite different things. Again, two children whose scores were far apart might be more alike, basically, than two children scoring about the same. The matter of children's conflicts—their fights and quarrels—offers an illustration.

In the records obtained in the spring, it was found that two children, one a boy and the other a girl, showed an increase in conflicts as compared with their records in the fall. But, although similar on the surface, in this respect, the story in each case was quite different:

One child—a boy—whom we will call Holden, was a youngster who in the fall stood far above the average in number of social contacts. He also exhibited few conflicts. Holden was rather small in size but quite resourceful. A large percentage of his contacts were with another child, Dennison, who acted pretty much as a willing slave. Then, during the middle of the school year, Holden was absent for some weeks. In the meantime, his loyal companion, partly with the help of the teachers, had found other friends. When the boy came back, he tried to renew his old relationship with Dennison. Dennison was still friendly—he tried to get Holden to join in play with his new friends, but he refused to follow Holden as he had done before. Holden, in spite of his apparently high social score in the fall, was not prepared to play with his pal and his pal's new friends on an equal basis of give-and-take. His old relationship with Dennison was destroyed, and he seemed for some time to lack the resources to reestablish himself. Then a number of things happened. During the fall and the preceding year he had been rather independent of adults, but now he hovered near the teachers a good deal of the time. While previously he had appeared to feel no need of affection, he now occasionally permitted such children as were willing to baby him, pet him and help him with his clothes. But the biggest change, as indicated, occurred in the matter of conflicts. His previous self-confident ways, which had worked with Dennison, did not work when he tried to join the other boys, most of whom were bigger than he, and who appeared to scorn his small size. They would reject him, yell, "Get out of here"—and when they did that, he would shout back to them and hit. When he hit, the others hit back. One record shows no less than four other boys pushing and hitting him at one time. He kept up the fight for a little while and then ran away crying. Fighting of this sort continued for some time during the spring and was responsible for the rise in his conflicts in the spring as compared with the fall.

The other case of a large increase in conflicts was the case of Mabel, who, in the fall, spent a large proportion of her time with a dominating companion carried over from the previous year. As time passed, she tried to break away from the sway of this companion, but the latter persisted in following along and importuning her. Mabel wanted to live her own life, and, to rid herself of these unwanted attentions, she began to fight. Her scores on conflicts went up. In this respect, she was like the boy we have just described. But the conflicts in Mabel's case fitted into quite a different context than did Holden's. When Mabel fought, she was trying to break loose from the domination of another child, and while fighting this child, she was widening her social activities and improving her adjustments within the group as a whole. In contrast, as long as Holden expressed his maladjustment by fighting, he was merely strengthening the barriers which he was trying to batter down.

The moral of this little nursery tale is apparent. It would be possible to add two such conflict scores together, along with others, as equivalent and constituent elements in an average. The average would have some meaning, since the conflicts in both cases were methods of solving a problem, but it would still give only a meager picture of underlying facts. This holds true from the point of view of understanding the children involved. It also holds true from the point of view of the teacher and her handling of the children.

Somewhat analogous conditions were found in other phases of the study, especially in the matter of individual differences in leadership among the children.

For example, there are two children who often directed the behavior of others. One of these children tried to rule by crude directions and commands, and while he frequently was obeyed, the other children soon left the scene. The other operated by invitation and suggestion more than by command; children not only followed his suggestions, but often sought him out. Following is a brief example from one of his records. Another boy has joined Kirk at a big box, which is a ship. Kirk establishes the boy on top of the box, then goes below and says: "I have to stay down here; I'm like an engineer." The other boy says, "I'm an engineer, too." Kirk says "You stay on top and you can be the captain. The engineer isn't the biggest boss. The captain is the biggest boss. Do you want to be the biggest boss?" The other boy says, "Yes." The game goes on, with the big boss standing in his glory on top, while Kirk, the humble engineer, really runs the ship and captain from down below. A tally simply of each time that a child such as this has his way with others and a tally of each time another child rules by browbeating methods, might give both children rather high scores, but such scores would not be as meaningful as they might be from the point of view of understanding children or from the point of view of the teacher.

Throughout the study, it appears as has been indicated, that children's adjustments and the effect of nursery school environment were highly individual matters. One child has been mentioned above, who, from the point of view of number of social contacts in the fall, might appear to be well adjusted, yet who early in the spring was in a rather sorry plight for some time. Both this child and one of the other "old" children went counter to the trend by showing a decline in social contacts from fall to spring, and neither of the two appeared to be well adjusted to other children or the nursery school situation.

On the other hand, there were two children who made a very small number of social contacts both in the fall and in the spring. But each of these, in his way, in the light of his record as a whole, appeared to have profited from the nursery school and to have achieved a better balance in his behavior, with much less sheer "orneriness" in nagging other children, in one case, and much less dependence on adults in the other.

On the whole, the results tend to agree with earlier findings to the effect that nursery school experience promotes the child's social development, although some children may fail to show much benefit, and some measures of apparent improvement may not mean as much as they seem to show.

In the course of the study, many observations were made of the practices of teachers and the effects of these practices on individual children. These observations were too limited to yield systematic findings, but, coupled with observations made in earlier investigations and in other groups, they do suggest the following comments. Nursery schools can be run too much on the theory that if only one throws a number of children together, gives them plenty of toys and equipment and good meals and naps, great benefits will come to the children who attend. Actually, many good results may come from this policy, but the writer believes that more could be done than usually is, if the teachers would use less of the hands-off policy and actually take more of a hand in helping individual children in specific ways. The nursery school offers many splendid opportunities to encourage skills and aptitudes that will help a child not only in coping with his physical environment but also make him more effective as a person, less timorous, and more independent. This does not imply that all children need be molded into a single pattern, with the robust, athletic, sociable type as a model. Jack's (5) investigation at the University of Iowa has suggested the manner in which training in a simple skill can have rather profound effects on a child's self-assurance and his relations with his fellows. Opportunities along this line are, in the writer's opinion, all too frequently neglected in preschool education.

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Two—Success and Failure in the Classroom

ROGER G. BARKER

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Barker's article, prepared primarily for teachers, is a thoughtful consideration of the psychological effects of academic failure upon the child. It is based upon several studies of the effect of success and failure upon the "level of aspiration."

Of the numerous rôles which the classroom teacher plays, that of dispenser of success and failure is undoubtedly the most impressive and worrisome to the pupils, and one of the most crucial for their present and future adjustment. It is also the rôle in which many teachers meet their severest conflicts; to fail John or not to fail him, whichever is done, frequently leaves feelings of guilt and anxiety. Clearly an understanding of the conditions and effects of success and failure would be of greatest value to teachers.

When does a child experience success? When does he experience failure? In what ways do these experiences affect behavior? Do the schools make it possible for children to achieve a sufficient number of important success experiences? If not, what can be done about it? A small but very important body of verified knowledge is now available bearing upon these crucial questions. In this article only a very small segment of these data can be presented.

Professor Kurt Lewin, then at the University of Berlin, and his student Ferdinand Hoppe initiated an experimental approach to these questions in the late 1920's (1). Hoppe first considered the fundamental problem of when a person experiences success and when failure. He presented his adult subjects with simple motor and intellectual tasks such as hanging 16 rings upon as many hooks as they passed upon a rapidly moving belt, and solving puzzles. During each trial with the tasks, Hoppe observed the subjects secretly and after the completion of each trial he interviewed them thoroughly in an effort to find out the circumstances under which they experienced success and failure. One result was clearly apparent: The experiences of success and failure were unrelated to the actual achievements of the individual. One subject might experience success when he placed 4 rings on the hooks; another experienced failure when he placed 15 correctly. In addition, for a particular person, the achievement experienced as success (or failure) continually changed; at one time a single ring correctly placed might give rise to an experience of success, while on a later occasion the placing of 6 rings would result in an experience of failure. These findings led Hoppe to a conclusion which seems very obvious once it is stated, but one that is so fundamental that it has very wide implications: The occurrence of success and failure experiences is independent of actual achievement; it is determined, rather, by the goals, expectations, and aspirations of the person at the time of the action. These expected achievements Hoppe called the level of aspiration.

It is obvious that the level of aspiration is important, for on it depends the occurrence of success and failure. Hoppe therefore directed his study to the effects of success and failure experiences on the level of aspiration. He found that after success the level of aspiration is usually raised (that is, a new and higher goal is set after a lower one is achieved), and that after failure of the level of aspiration is usually lowered (that is, a new and lower goal is set after a high one has not been achieved). He found, in other words, that the level of aspiration shifts in such a way that, whatever the actual achievement of the person, the frequency of his success and failure experiences remains fairly constant. This means that the level of aspiration operates as a mental hygiene factor of great significance. It constitutes a sort of governor; it protects the person against continual failure on the one hand, and against easy achievements which do not give the feeling of success, on the other hand. This fact is behind the frequent observation that feelings of success accompany the proceess of achieving but disappear after attainment.

Sometimes, however, this mechanism is thrown out of balance and it fails to perform this protective function. In some cases, aspirations are maintained consistently above achievement. The individual is then subjected to continual failure with its disastrous consequences for adjustment and happiness. In other cases, aspirations are placed consistently below achievement with resulting lack of ambition, exaggerated caution, broken morale, cynicism, and so forth. In both instances very serious personal and social difficulties may develop.

It is of the greatest importance, therefore, to determine why the level of aspiration does not function protectively for these persons.

Hoppe suggested that the level of aspiration is set as a compromise between two conflicting tendencies: (a) the desire to avoid the hurt accompanying failure, operating to force aspirations safely below the level of achievement; and (b) the desire to succeed at the highest possible level; operating to push goals above achievement levels. Subsequent investigations suggested that the latter tendency derives from social pressures to do what is most highly approved by society, irrespective of a realistic assessment of one's own capabilities. This conflict between fear of failure and desire to maintain goals that are socially approved results, usually, in a level of aspiration at or near the upper limit of one's ability range.

If this interpretation is correct, it would be expected that an increase in social pressure should alter the level of aspiration. This is, in fact, the case. Subsequent investigations have shown that pupils at the low end of the class achievement distribution aspire, on the average, above the level of their achievement possibilities (and therefore experience failure), while those at the upper end of the achievement distribution set their aspirations below their level of achievement (and therefore experience success).

Although the differences between aspiration and achievement are not great in a quantitative sense, they are psychologically very important. So far as success and failure are concerned, "a miss is as good as a mile." This difference in relation of aspiration to achievement appears to mean that the social pressures of the school situation may operate to throw off balance the protective mechanism of the level of aspiration, thus subjecting children to exaggerated failure and success experiences.

It is not difficult to understand why these pressures arise in many schools. Social acceptability in an intimate group such as a school class requires a high degree of conformity to group standards in all sorts of public behavior. The first step in achieving such acceptability is to set goals in accordance with the group standards. In schools where evaluation is largely on the basis of academic achievements this means that poor students are forced, by the social pressure of the classroom, to admit that they are mavericks; both are undesirable alternatives from a mental hygiene viewpoint. There is pressure upon bright students, also, to set their goals in conformity with the achievements of their roommates, rather than with their own.

Adults on the other hand are infrequently subjected to such pressures for long periods of time, for adults are able with considerable success to hide from others certain crucial symbols of their divergence from what is considered good or desirable (such as age, income, family background), and they are able to withdraw when the pressures become too great. Furthermore, achievement in most adult activities is not estimated with the precision that is attempted in many schools. Doctors, lawyers, plumbers, and bakers can vary within a considerable range of effectiveness and no one is wiser; they are still adequate. This gives a fundamental security which is denied to pupils who are frequently and publicly evaluated, that is, acclaimed or humiliated by an authority from whose decisions there is no recourse and in a group from which there is no escape.

Middle-class pupils are unusually sensitive to these pressures. They are, in effect, subjected to the demands of a single dominating institution, for the family supports the demands of the school. This means that the pressures, the demands, the rewards, the punishments, the successes, and the failures of the school are frequently of overwhelming importance to these children. No one with influence will question the righteousness of the school's verdicts or the correctness of its values. If the school is one in which the rewards are all centered about a very limited variety of achievements, for example, academic achievements, the child who is relatively dull or uninterested in academic activities must experience continual failure. He will fail even though he is kind, or good looking, or has a sense of humor, or has physical prowess, even though he is full of energy, graceful, courageous, friendly, or with mechanical abilities. He will fail in school even though these behavior characteristics are very highly valued by many other institutions, until in adolescence he becomes sufficiently independent to establish affiliations with other groups which do reward nonacademic achievement.

Compared with life outside school, many schools distribute success and failure in an extremely unrealistic way. Adults, for example, are inevitably influenced by various pressures, and rewarded according to conflicting values of a variety of institutions and social groups (family, vocation, clique, church, lodge, union, and so on), and these influences are likely to be of somewhat equal potency in their lives. This means that the adult can to some extent balance the failures in one region of his life by successes in other regions. The effects of vocational failures may be mitigated by successes in family and recreational relationships where quite different achievements are valued. In schools that emphasize academic achievement, this kind of balancing is impossible for middle-class children.

What is the consequence of the chronic failure and success that many schools enforce upon great numbers of pupils? We do not know a great amount from scientific experiment but what we do know is very suggestive.

Sears (2) studied the level of aspiration of a group of fifth grade children who had long histories of chronic school failure in reading and arithmetic, and another group with equally consistent histories of school success in reading and arithmetic. She found that the children who had experienced continual success set their aspirations at a realistic level, that is, at a level where success was frequently achieved. The children with a history of chronic failure, on the other hand, set their aspirations with little regard for their achievements. Of those in this latter group, some children apparently lived almost exclusively in terms of their aspirations, ignoring completely the fact that their achievements were entirely out of line with their expectations. In these cases the desire for respectability may have forced the children to an imaginary world where the mere gesture of achieving by setting high goals was accepted in lieu of real achievement. The seriousness of this behavior is sufficiently obvious to need no special emphasis. The institutionalized person for whom a gesture is sufficient to convince him he is Napoleon has traveled further along the same path.

The cases where the children failed to set goals even at the level of their poor achievement may involve withdrawal from the activity in any except a very peripheral sense; they may be cases of extreme caution or they may represent attempts to depreciate the importance of the activity by refusing to take it seriously. None of these outcomes of educational effort are desirable.

What can schools do to avoid throwing out of gear the protective mechanism of the level of aspiration with the resulting unfortunate consequences for the success and failure experiences of pupils? The answers are implied in the discussion, but they may be summarized as follows: (a) broaden the basis for evaluating pupils; (b) reduce to a minimum the prominence of the relative standing of the pupils; (c) allow maximum freedom to pupils to set their own goals and to alter them as their success and failure experiences require; that is, make success possible at all levels of achievement; (d) reduce the dominance of the teacher.

These conditions can be achieved in different ways. It is interesting to note, however, that they can hardly be avoided if democratic teaching procedures are used, if the interests of the child are followed, and if group undertakings are an important part of school activities.

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Three—Teachers and Behavior Problems

E. K. WICKMAN

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The section which follows is a condensation, prepared by Wickman, of an extensive study that he published under the title of CHILDREN'S BEHAVIOR AND TEACHERS' ATTITUDES. *It is a challenging statement, indicating that to many teachers, quiet industry and obedience on the part of the pupil are of major importance and that serious problems are frequently not perceived by the teacher.*

What identifies the problem child? How do we determine that a child is well adjusted, or maladjusted? What kinds of behavior are undesirable in any child? How "normal" is misbehavior in children?

The answers that teachers make to these questions are the subject of this study.

However perplexing the question may be to modern parent or teacher, the immediate requirements of child rearing and training necessitate definite answers. Such answers are to be found in the direct responses of adults to the distressing behavior of their children. Some customary responses to child behavior have lately been called in question, subjected to careful examination, and are being modified according to our growing knowledge of child life and child needs. But many everyday habits of regarding and treating problems in child behavior are so taken for granted that they are rarely formulated or held up to intellectual scrutiny. This study is an effort to analyze teachers' attitudes toward behavior problems of children.

The subject of child behavior has recently taken on a new significance. The relationship of behavior disorders of children to problems of adult mental health and social adjustment is inviting careful attention. The importance of the emotional and social development of children is becoming recognized along with the need for their intellectual and physical training. Education is turning serious attention to preparing the child for life. With our concepts of child needs and child problems in a state of reorganization, it is appropriate to inquire at what point we have arrived in defining the issues of child behavior in our everyday practices.

The study of teachers' attitudes toward child behavior was an unexpected outgrowth of experimental investigations conducted in 1924-1926 in two large elementary public schools, one in Minneapolis, the other in Cleveland. Originally the object of the investigations was to secure information on the incidence of behavior problems in elementary school children. To this end, three steps in procedure were undertaken. *First*, the teachers listed all the kinds of problems in behavior which they had encountered in their teaching experience. *Second*, using the compiled list of behavior problems which they had submitted, the teachers checked the frequency of the occurrence of the problems in each of their pupils. *Third*, the teachers rated the total behavior adjustment of each child.

As the investigation progressed, it became increasingly apparent that the teachers were preoccupied with behavior difficulties exhibited by one general type of problem child and were little concerned with personality and behavior characteristics of another type of maladjusted child. This unanticipated finding seemed so important that the investigation was forthwith directed to an inquiry into teachers' attitudes toward children's behavior; and a *fourth* step in procedure was undertaken by which ratings of the seriousness of various behavior problems were elicited from many teachers and from a number of mental hygienists.

What the findings in each of the four steps of procedure reveal with reference to the attitudes of teachers toward the behavior problems of children are briefly summarized in the following four sections. For a full account of the methods of investigation and the results, the reader is referred to *Children's Behavior and Teachers' Attitudes.*

TEACHERS' DESIGNATIONS OF UNDESIRABLE BEHAVIOR

When the teachers in the two elementary schools were asked to record the behavior problems in children which they had encountered in their teaching careers, they submitted a list covering a wide range of problems. However, there appeared a definite tendency on the part of teachers to stress behavior disturbances that attack their standards of morality, obedience, orderliness, application to school work, and agreeable social conduct. Even the personality traits enumerated were limited almost entirely to those which are disagreeable and annoying. On the other hand, there was a conspicuous paucity of items describing child behavior which does not directly disturb school routine but which is indicative of social and emotional problems. Those individual problems of the child which do not cause active annoyance to the teacher appeared only incidentally in the reports.

The individual teachers tended to stress specific types of difficult behavior, but appeared to be concerned mostly either with the stubborn, the disorderly, the irresponsible, the untruthful, or the disobedient child. A few reports from individual teachers are here reproduced to illustrate these individual differences.

The teacher who submitted the following list seemed to be chiefly concerned with problems of obedience and honesty. Note that the first seven items are all descriptive of a pupil's antagonism toward the teacher's authority:

Determination to have own way
Contrariness
Antagonistic attitude toward teacher and others in authority
Disobedience
Stubborness
Conceitedness
Self-pity, feeling of not getting a square deal
Indifference
Laziness
Dishonesty
Tendency to lie
Sneakiness
Tendency to steal

Another teacher apparently concentrated attention on problems which offended her standards of morality. Her entire list was as follows:

Stealing of money and such things as pencils and pens
Telling lies
Cheating
Swearing
Telling obscene stories
Writing obscene notes
Drawing obscene pictures
Smoking cigarettes, chewing tobacco, drinking whiskey and liquor
Destroying property wilfully
Carrying dangerous weapons

Bullying smaller boys and molesting girls
Always quarreling with other children
Truancy
Laziness
Sullenness
Insolence
Running away from home
Ungovernable temper
Cruelty, such as tying cans to cats' tails, getting dogs to chase cats, getting dogs to fight each other, killing birds and stealing birds' eggs

There is little doubt as to the type of discipline maintained by the teacher who submitted the following:

Telling an untruth
Copying from some other person's paper
Truancy
Laughing at others' mistakes
Excusing themselves from trouble by blaming someone else
Whispering when child should be working
Not putting pens down when told in writing lesson
Raising hands when standing
Standing on wrong side of seat when ready to answer
Not standing still when ready to answer
Casting side glances to another child
Snickering
Noise when getting books out of desk for next class
Turning around in seat
In a music lesson humming or singing aloud when asked to study
Wasting time when coming into the room
Writing notes
Playing with pencil, ruler, etc.
Disorderly lines when passing
Asking to leave room when not necessary
Not getting to work upon coming into the room
Talking when not being called upon
Erasing board when asked to leave work there
Talking in the hall
Talking aloud in library
Unnecessary noise in coming up steps
Failure to bring required materials
Failure to have necessary materials ready for work
Watching other people instead of taking care of own work

The suggestive findings obtained from this initial approach to the study of teachers' attitudes toward child behavior raised important questions for further investigation for which more refined methods of inquiry were necessary. For example: Are behavior problems in school children diagnosed by the immediate effect of the behavior on the teachers? Is the problem child in school identified chiefly by annoying, aggressive disobedient behavior? Are problems of shyness, dependency, and retirement in children regarded by teachers as important problems in behavior?

TEACHERS' REPORTS ON THE BEHAVIOR OF PUPILS

Answers to such questions began to appear when teachers reported the occurrence of problems in their individual pupils. Table I, which shows the reported incidence of some 50 behavior problems in the children of the Cleveland school, indicates definite trends in the teachers' awareness of the various behavior disorders.

In the group of problems in the upper third of the chart—those reported most frequently by the teachers—appear all the behavior items that describe violations of specific classroom rules and children's failure to meet prescribed standards of school work. The teachers, it will be observed, complained that nearly 60 per cent of their pupils were at times inattentive, over 40 per cent careless in their work, and almost the same number failed to study. One out of every three children did not always show proper interest in school tasks. Overactivity, restlessness, and inability to work calmly were reported for 30 per cent, laziness for 20 per cent. Approximately one-half of the children reported for each of these problems were said to be frequent or habitual offenders. Nearly 4 out of every 10 pupils were described as being disorderly at one time or another "by not complying with the established order and routine"; and well over half of all children so reported were described as frequent or constant offenders.

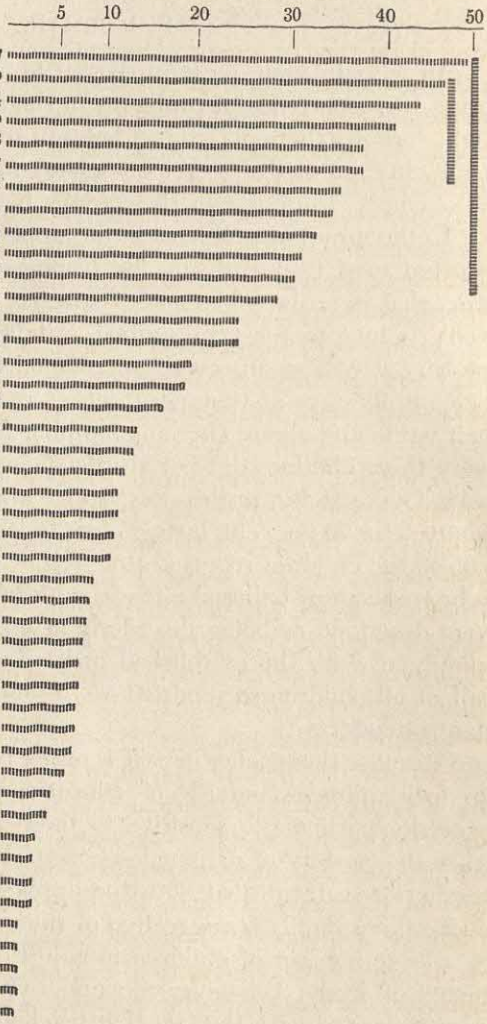
Because the teacher herself is under pressure to bring her pupils up to required standards of educational achievement, she is of necessity particularly sensitive to those problems in children that frustrate mastery of assigned school tasks. However, the fact that teachers are dissatisfied with the application of nearly one out of every three pupils is suggestive of inherent difficulties on the part of a large number of children in adjusting to the imposed requirements of study. Whatever the underlying difficulties may be, we are confronted with the fact that teachers' attitudes contain a large measure of dissatisfaction with the school work of their pupils which cannot fail in turn to influence the attitudes of the children themselves toward school and all that the school symbolizes in their developmental life.

In the upper third of the behavior items in the chart appear also the three problems of shyness, sensitivity, and daydreaming.

TABLE I

INCIDENCE OF BEHAVIOR PROBLEMS REPORTED BY TEACHERS OF AN ELEMENTARY PUBLIC SCHOOL, CLEVELAND, 1926

Type of Problem	*Percentage of Pupils*
Whispering	74.7
Inattentive	59.0
Careless in work	44.4
Tattling	42.0
Disorderly in class	38.8
Interrupting	38.7
Failure to study	36.2
Shy, withdrawing	35.2
Daydreaming	33.4
Lack of interest	31.8
Overactive	30.9
Cheating	29.5
Oversensitive	25.5
Neglectful	25.4
Physically lazy	20.8
Lying, untruthful	19.6
Unnecessary tardiness	17.6
Acting "smart"	14.6
Overcritical	14.2
Imaginative tales	13.3
Meddlesome	12.6
Sullen, sulky	12.5
Domineering	12.1
Slovenly appearance	11.8
Suggestible	9.4
Fearful	9.3
Physical coward	8.8
Nervous	8.7
Wilfully disobedient	8.2
Destroying property	8.2
Unhappy, depressed	8.0
Quarrelsome	7.9
Stubborn in group	7.5
Rude, impudent	6.7
Impertinent, defiant	5.6
Carrying grudges	4.9
Stealing articles	4.0
Masturbation	3.9
Enuresis	3.9
Sissy (or tomboy)	3.6
Suspicious	2.1
Cruel, bullying	1.7
Profanity	1.7
Truancy	1.6
Temper outbursts	1.5
Stealing money	0.7
Stealing food, sweets	0.7
Obscene notes, talk	0.3
Smoking	0.2



Though such behavior was reported for a fairly large number of children, it was found, later in the study, to be judged by the teachers as of little importance from the standpoint of child adjustment. In this connection, also, it will be observed that teachers seem to be more aware of problems of dishonesty than they are of specific emotional and neurotic difficulties. Only 8 per cent of the children were thought by their teachers to be unhappy, depressed, or dissatisfied with themselves. These personal problems were reported to occur frequently in only 12 out of 871 pupils. Moreover, from the teachers' ratings of the seriousness of problems observed in their pupils, it developed that only one teacher in the Cleveland school was seriously concerned about the emotional problems of a single pupil. On the other hand, while the teachers also found only about 8 per cent of their pupils wilfully disobedient, they regarded most of these as serious problems.

In line with this finding is the fact that the teachers reported many more problems for boys than for girls, indicating that they are particularly sensitive to aggressive, overt forms of behavior problems.

Though the teachers in the Cleveland school had fairly homogeneous classes, they revealed considerable individual differences in their reports on the occurrence of problems in their children. Thus, disorderliness in the classroom was reported by two teachers of the Cleveland school to occur in 100 per cent of their pupils, while seven other teachers observed it in less than 15 per cent of their children. One teacher found 92 per cent of her pupils dishonest, while two declared they had observed no instances of untruthfulness in any of their children. Three reported wilful disobedience in over one-third of their pupils, while nine teachers had never observed it in a single child. Four declared that over two-thirds of their children cheated but six others believed none of their pupils guilty of this conduct. Shyness was observed in over two-thirds of the children in the classes of four teachers; while according to another teacher it was entirely absent in all her pupils.

Despite such individual differences in sensitivity to problem behavior, the reports of the teachers as a whole indicated that the problems which most engage their attention are those which relate to the school situation, namely, infractions of classroom rules and routine, and failure to meet school work requirements. The personal problems of the child seem to be subordinated to the problems encountered in teaching and in classroom management. Of these personal problems, those which are expressed in overt and directly annoying behavior are more firmly registered in the minds of teachers than are the inner emotional conflicts of children.

TEACHERS' AND MENTAL HYGIENISTS' ATTITUDES TOWARD BEHAVIOR PROBLEMS

As the evidence of a predominating trend in attitudes toward children's behavior accumulated in the three previous steps in the investigation, it became worth while to determine whether or not the findings were typical of the attitudes of teachers generally. A fourth step in procedure, therefore, was taken to test the attitudes of many teachers. The test was made by use of a rating scale on which teachers recorded their convictions about the relative seriousness of the same 50 behavior problems. Ratings were secured from 500 teachers who composed the entire teaching staffs of 13 elementary public schools in New York, New Jersey, Ohio, and Minnesota.

The results, shown graphically in Table II, fully corroborated the previous findings.

As a final step in procedure, the teachers' reactions to the behavior problems were compared with the clinical judgments of mental hygienists* about the seriousness of the same problems. Their ratings are shown in Table III. There was this difference in securing the ratings of the two professional groups: In testing the attitudes of teachers, effort was made to elicit so far as possible their everyday reactions to these problems; but in testing the opinions of the mental hygienists, the object was to elicit their intellectual judgments about the relative seriousness of the problems in so far as they may be indicative of maladjustment. It is this comparison of the customary modes of response of teachers with the considered opinions of mental hygienists that is of primary importance in this study.

The most striking difference between the teachers' and the mental hygienists' ratings is to be found in the comparative estimates of problems describing the withdrawing, recessive personality and behavior traits. Whereas teachers considered shyness, sensitiveness, unsocialness, fearfulness, dreaminess among the least serious of all problems, the mental hygienists ranked them together with unhappiness, depression, easy discouragement, resentfulness, cowardliness,

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* Ratings were secured from 30 psychiatrists, psychologists, and psychiatric social workers, who composed the entire clinical staffs of two child guidance clinics in Cleveland and Philadelphia, and the Department of Child Guidance in the Newark Public Schools.

TABLE II

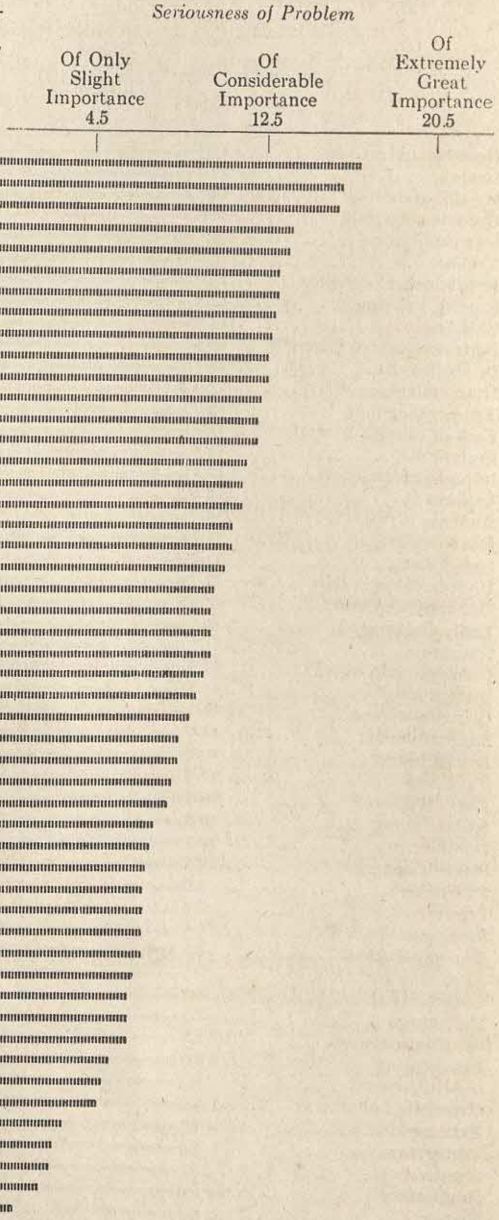
TEACHERS' RATINGS ON THE RELATIVE SERIOUSNESS OF BEHAVIOR PROBLEMS

Type of Problem	*Average Score*	*Seriousness of Problem*		
		Of Only Slight Consequence 4.5	Makes for Considerable Difficulty 12.5	An Extremely Grave Problem 20.5
Heterosexual activity	17.5			
Stealing	17.0			
Masturbation	16.7			
Obscene notes, talk	16.6			
Untruthfulness	15.8			
Truancy	15.6			
Impertinence, defiance	15.0			
Cruelty, bullying	14.8			
Cheating	14.7			
Destroying school materials	14.3			
Disobedience	14.1			
Unreliableness	13.9			
Temper tantrums	13.0			
Lack of interest in work	12.8			
Profanity	12.3			
Impudence, rudeness	12.2			
Laziness	12.2			
Smoking	12.0			
Enuresis	11.8			
Nervousness	11.7			
Disorderliness in class	11.7			
Unhappy, depressed	11.5			
Easily discouraged	11.5			
Selfishness	11.3			
Carelessness in work	11.3			
Inattention	11.2			
Quarrelsomeness	11.1			
Suggestible	11.0			
Resentfulness	10.8			
Tardiness	10.5			
Physical coward	10.4			
Stubbornness	10.3			
Domineering	10.3			
Slovenly in appearance	10.1			
Sullenness	9.9			
Fearfulness	9.7			
Suspiciousness	9.1			
Thoughtlessness	8.7			
Attracting attention	8.5			
Unsocialness	8.3			
Dreaminess	8.3			
Imaginative lying	8.1			
Interrupting	8.0			
Inquisitiveness	8.0			
Overcritical of others	7.9			
Tattling	7.5			
Whispering	7.5			
Sensitiveness	7.0			
Restlessness	6.9			
Shyness	5.4			

TABLE III

MENTAL HYGIENISTS' RATINGS ON THE RELATIVE SERIOUSNESS OF BEHAVIOR PROBLEMS

Type of Problem	*Average Score*
Unsocialness	17.3
Suspiciousness	16.4
Unhappy, depressed	16.2
Resentfulness	14.1
Fearfulness	14.0
Cruelty, bullying	13.5
Easily discouraged	13.4
Suggestible	13.3
Overcritical of others	13.2
Sensitiveness	13.1
Domineering	13.0
Sullenness	12.6
Stealing	12.5
Shyness	12.5
Physical coward	12.0
Selfishness	11.8
Temper tantrums	11.7
Dreaminess	11.3
Nervousness	11.3
Stubbornness	10.9
Unreliableness	10.4
Truancy	10.3
Untruthfulness	10.3
Cheating	10.3
Heterosexual activity	9.9
Lack of interest in work	9.6
Enuresis	9.2
Obscene notes, talk	8.8
Tattling	8.8
Attracting attention	8.5
Quarrelsomeness	8.3
Impudence, rudeness	7.6
Imaginative lying	7.5
Inattention	7.3
Slovenly in appearance	7.2
Laziness	7.2
Impertinence, defiance	7.1
Carelessness in work	7.1
Thoughtlessness	6.8
Restlessness	6.4
Masturbation	6.4
Disobedience	6.4
Tardiness	5.6
Inquisitiveness	5.3
Destroying school materials	5.1
Disorderliness in class	3.4
Profanity	2.9
Interrupting	2.8
Smoking	2.3
Whispering	0.8



suggestibility, and overcriticalness, at the very top of the list as the most serious problems. These items in the mental hygienists' ratings completely replace the problems relating to sex, dishonesty, and disobedience which the teachers ranked as most serious.

The items describing defiance to authority—impertinence, impudence, disobedience—which teachers considered very serious appear near the bottom of the mental hygienists' list. Truancy, together with problems of dishonesty which constitute grave offenses in the school, was rated significantly lower by the mental hygienists about midway in the scale of seriousness. Problems relating to difficulties in adjusting to school work—lack of interest, laziness, carelessness, inattention—were all reduced in degree of seriousness by the mental hygienists, though not ranked so low as the items of disobedience and disorderliness. Problems designating failure of pupils to follow classroom discipline, like disorderliness and whispering, were also greatly discounted in importance by the mental hygienists who rated them as the least serious of all problems.

The direction of teachers' reactions to the seriousness of behavior disorders of children may be expressed (by borrowing the mathematical symbol > for designating inequalities) as follows:

TEACHERS REGARD

Most serious						*Least serious*
Stealing Untruthfulness Cheating Sex problems Disobedience Impertinence, defiance Temper outbursts Impudence, rudeness Truancy	>	Disorderliness in class Inattentiveness Lack of interest in work Carelessness Laziness Unreliableness	>	Domineering Attracting attention Sullenness Interrupting Meddlesomeness	>	Shyness Unsocialness Sensitiveness Fearfulness Suspiciousness Imaginative lying Dreaminess

By interpretative classification of the groups of problems, the above formulation becomes:

TEACHERS REGARD

Most serious						*Least serious*
Immoralities Dishonesties Transgressions against authority	>	Violations of orderliness in classroom application to school work	>	Extravagant, aggressive personality and behavior traits	>	Withdrawing, recessive personality and behavior traits

The ratings made by the mental hygienists are not quite as well arranged according to types of problems; but we may formulate the direction of their reactions in contrast to the teachers' reactions, as follows:

MENTAL HYGIENISTS REGARD

Most serious						*Least serious*
Withdrawing, recessive personality and behavior traits	>	Dishonesties Cruelty Temper tantrums Truancy	>	Immoralities Violations of school work requirements Extravagant behavior traits	>	Transgressions against authority Violations of orderliness in class

Four—When Should Children Begin to Read?

MABEL V. MORPHETT &
CARLETON WASHBURN

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The grade placement of most school subjects is a result of tradition and of "experience." The study here reported is one of the few that have been conducted to determine the results of beginning a specific educational procedure at different age levels. Obviously, education is greatly in need of such research as a guide to its efforts.

In tracing back to their origins the reading difficulties of some children and their distaste for the subject, the Department of Educational Counsel in Winnetka found that in several instances the children's mental ages on entering the first grade had been low and that discouragement had resulted from their first attempts to learn to read. This discouragement sometimes resulted in a mental set against reading, which lasted for years and which hampered all their school work. The research department, therefore, with the aid of the primary grade teachers, set about the task of discovering the period in the mental development of children when, as a rule, there is the best chance of their learning to read readily.

In September, 1928, all Winnetka first grade children, 141 in number, were given the Detroit First-Grade Intelligence Test. The eight first grade teachers were not told the mental ages of the children and attempted to teach all of them to read. The method, in accordance with the Winnetka technique, was largely individual, so that the slow children did not retard the fast ones. In February, 1929, the reading progress of these children was measured for the purpose of determining the amount of progress made by children at each mental level.

In order that the reading progress might be measured, the first large teaching unit was divided into definite steps, which were measurable by the teachers. Twenty-one steps took the children through the beginning reading materials.* Each further step represented the reading of a primer or first reader. Reading progress was measured by the number of these steps which the child had completed by February.

In addition to these progress steps the sight-word score of each child was measured. Each child in Winnetka is required to know at least 139 words at sight before passing from first grade reading to second grade reading. These words are those most frequently used in primers and first readers. The children were tested individually with flash cards, and the number of words recognized by each child was recorded as his sight-word score. In some cases the children knew some of the second grade sight words as well as the 139 first grade words. In such cases the score was the total number of first and second grade words recognized.

The first grade teachers, all of whom had had several years of experience with the reading materials, agreed that children who seemed ready for reading from the beginning of the year had usually completed at least 13 progress steps and knew at least 37 sight words by February. Therefore, 13 progress steps and 37 sight words were accepted as the measure of the minimum degree of satisfactory progress.

The Detroit First-Grade Intelligence Test and the Standford Revision of the Binet-Simon Scale were used to determine the mental ages of the children. The Detroit tests were given to all first grade children entering in September. The Stanford-Binet test was given later in the year, and the mental ages were calculated as of September, 1928. In this way comparison between the mental ages determined by the Detroit and Stanford-Binet tests was made possible.

Table I gives the correlations which were found between the sight-word scores and intelligence and between reading progress and intelligence. Since the data proved to be nonlinear, the correlation ratios rather than the correlation coefficients are given. When the relation between reading progress and intelligence was calculated, it was necessary to use the rank method of figuring correlations since the intervals of progress were not necessarily of equal difficulty. The correlations show that there is a fairly high degree of relationship between mental age and reading progress. The Detroit test shows more relation to progress than does the Stanford-Binet test.

* Livia Youngquist and Carleton Washburne, *Winnetka Primary Reading Materials*. Chicago: Rand McNally & Co., 1928.

Of the three measures of intelligence—mental age, average of the mental and chronological ages, and intelligence quotient—mental age shows the greatest degree of relationship although the differences are slight. In all the calculations that follow, mental age alone is used as the method of figuring intelligence.

TABLE I

Correlations between Achievement in Reading of 141 First-Grade Children and Their Intelligence as Measured by Detroit Test and Stanford-Binet Test

Factors Correlated	*Detroit Test*	*Stanford-Binet Test*
Sight-word score and mental age	.65	.58
Sight-word score and average of mental and chronological age as of September 1, 1928	.57	.49
Sight-word score and intelligence quotient	.56	.54
Reading progress and mental age	.59	.51
Reading progress and average of mental and chronological age as of September 1, 1928	.55	.49
Reading progress and intelligence quotient	.50	.53

TABLE II

Number of Children of Each Mental Age and Percentage Making Satisfactory Reading Progress

Mental Age in Years and Months *	*Number of Children* †		*Percentage Making Satisfactory Reading Progress* ‡	
	Detroit Test	Stanford-Binet Test	Detroit Test	Stanford-Binet Test
4–5 to 4–11	1	1	....	....
5–0 to 5–5	12	1	0	....
5–6 to 5–11	12	12	0	8
6–0 to 6–5	17	22	47	41
6–6 to 6–11	23	38	78	68
7–0 to 7–5	29	31	79	68
7–6 to 7–11	16	15	75	87
8–0 to 8–5	7	11	....	82
8–6 to 9–0	8	2	....	....

* Intervals are half sigmas above and below the mean of the entire group as determined by the Detroit test.

† Because the tests were given on different dates, some children who were given the Detroit test were not given the Stanford-Binet test and *vice versa*.

‡ No percentages were figured for groups of less than ten children.

The scores were next divided into groups based on the children's mental ages in September. The percentage of Children of each mental age making satisfactory progress (13 steps or more) and the percentage making satisfactory sight-word scores (37 or more) were determined. Tables II and III show the results.

Table II shows that a small percentage of children who began reading with a mental age of less than six years were able to achieve satisfactory reading progress but that for the group having a mental age between six years and six months there was a sharp rise in the percentage making satisfactory progress. This fact is shown graphically in Figure 1. The curves for the Stanford-Binet and the Detroit tests are essentially alike, although final flattening occurs later on the Stanford-Binet curve.

TABLE III

NUMBER OF CHILDREN OF EACH MENTAL AGE AND PERCENTAGE MAKING SATISFACTORY SIGHT-WORD SCORES

Mental Age in Years and Months	*Number of Children**		*Percentage Making Satisfactory Sight-Word Scores*	
	Detroit Test	Stanford-Binet Test	Detroit Test	Stanford-Binet Test
4-5 to 4-11	1	1	....	....
5-0 to 5-5	12	1	0	....
5-6 to 5-11	12	12	0	8
6-0 to 6-5	17	25	71	52
6-6 to 6-11	23	43	87	77
7-0 to 7-5	31	35	84	89
7-6 to 7-11	23	18	83	94
8-0 to 8-5	10	11	90	91
8-6 to 9-0	12	3	100	....

* The numbers of children whose sight-word progress is compared differ from the numbers whose reading progress is compared in Table II because one group of children not taught by the individual method was omitted from the reading progress group.

The curve of the results on the Stanford-Binet test seems to indicate that children would gain considerably in speed of learning if they could wait until they had attained a mental age of seven years and six months before beginning to read. However, the curve of the results of the Detroit test shows that the children with mental ages of six years and six months made progress practically as satisfactory as that of the children with higher mental ages. Since the results of the Detroit test show a higher correlation with reading progress than do the results of the Stanford-Binet test and since the Detroit test is more practicable to administer than the Stanford-Binet test, it seems reasonable to use the Detroit test as a basis for determining children's readiness for reading. The mental level of six years and six months is the breaking point in the curve, that is, the point beyond which there is very little gain in postponing the teaching of reading. This break is evident to some extent on the

Stanford-Binet curve and markedly true on the curve of Detroit test scores.

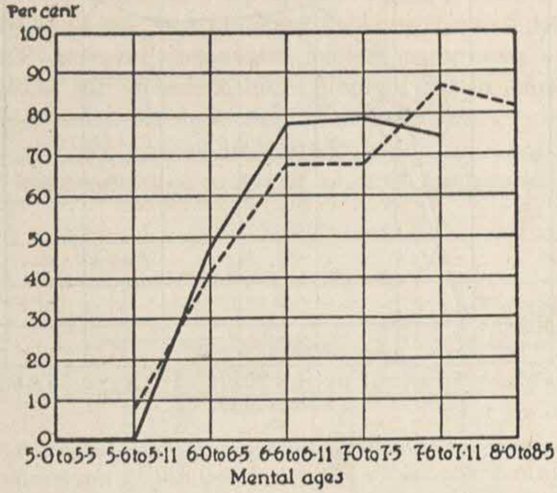

Figure 1. Percentages of children of various mental ages, as determined by the Detroit First-Grade Intelligence Test (solid line) and by the Stanford Revision of the Binet-Simon Scale (broken line), making satisfactory reading progress in the school year, 1928-29.

Figure 2 points to the same conclusion—that it pays to postpone beginning reading until a child has attained a mental age of six years and six months. If this practice is followed, 78 per cent of the children may be expected to make satisfactory general progress, and 87 per cent of the children may be expected to make satisfactory progress in learning sight words.

A similar study was carried on during the school year 1929-30 for the purpose of checking the results of the 1928-29 experiment.

All children who were mentally six years of age or more were taught reading from the beginning of the year. The previous study made it seem futile to try to teach younger children, but a few with lower mental ages were taught reading for the purpose of the experiment. Mental ages were determined this time by the Detroit First-Grade Intelligence Test and the Pintner-Cunningham Primary Mental Test.

At the end of the year (June, 1930) the children were tested on the sight-word list and the Gray Standardized Oral Reading Check Test. A child was considered to have made satisfactory progress if he knew the entire sight-word list (139 words) and read the Gray test in 50 seconds or less with 3 errors or less. This standard has

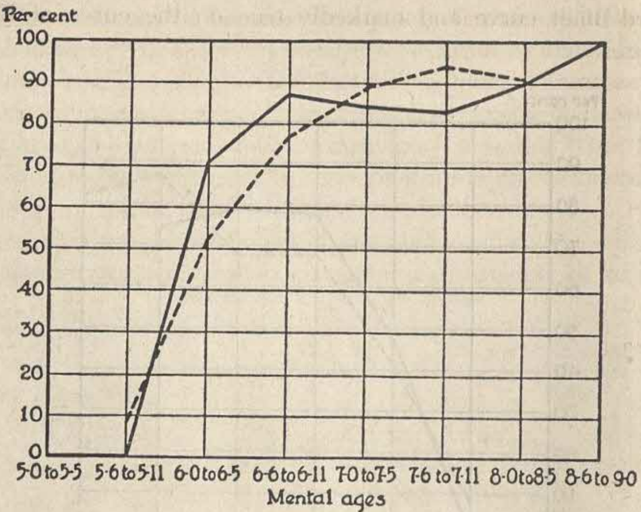

Figure 2. Percentages of children of various mental ages, as determined by the Detroit First-Grade Intelligence Test (solid line) and by the Stanford Revision of the Binet-Simon Scale (broken line), making satisfactory sight-word scores in the school year, 1928-29.

been set by Gray for Grade I. Table IV gives the number of children of each mental age and the percentage of children at each mental level making satisfactory scores in both sight words and oral reading. Figure 3 makes the data of Table IV graphic. As in Figures 1 and 2, the percentage of children who learned to read satisfactorily is

TABLE IV

NUMBER OF CHILDREN OF EACH MENTAL AGE AND PERCENTAGE MAKING SATISFACTORY SIGHT-WORD SCORES AND ORAL READING SCORES

*Mental Age in Years and Months**	*Number of Children*		*Percentage Making Satisfactory Progress*	
	Sight-Word Test	Oral-Reading Test	Sight-Word Test	Oral-Reading Test
5-0 to 5-5	1	0	....	....
5-6 to 5-11	10	9	....	....
6-0 to 6-5	25	24	64	58
6-6 to 6-11	23	23	87	83
7-0 to 7-5	23	23	87	91
7-6 to 7-11	12	12	83	92
8-0 to 8-5	5	5	....	....
8-6 to 9-0	1	1	....	....

* Average of scores on Detroit test and Pintner-Cunningham test.

greatest at the mental ages of six years and six months and of seven years. The curve for sight-word scores breaks at the mental age of six years and six months, while the curve on the Gray Standardized Oral Reading Check Test breaks at the mental age of seven.

The second year's experiment, therefore, in which a different set of children, different teachers, a different method of determining mental age, and a different method of determining progress were used and in which a whole year's work instead of a half year's was taken as the measure of progress confirms the experiment of the first year.

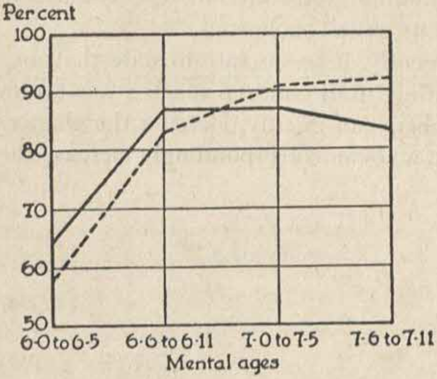

Figure 3. Percentages of children of various mental ages who, in school year 1929-30, made satisfactory sight-word scores (solid line) and satisfactory scores on the Gray Standardized Oral Reading Check Test (broken line).

SUMMARY

1. Correlations between mental age and ability to learn to read, as measured by reading progress and sight-word scores, showed a fairly high degree of relationship. The correlations ranged from .50 to .65.

2. The correlations between mental age and reading progress were somewhat higher when mental age was measured by the Detroit First-Grade Intelligence Test than when mental age was measured by the Stanford Revision of the Binet-Simon Scale.

3. Mental age alone showed a larger degree of correlation with reading progress than did the intelligence quotient or the average of mental and chronological ages.

4. When the Detroit test was used as a basis for determining mental age groups, the children who had a mental age of six years and six months made far better progress than did the less mature

children and practically as satisfactory progress as did the children of a higher mental age.

5. When mental age was measured by the Stanford Revision of the Binet-Simon Scale, the children with a mental age of six years and six months again made very much better progress in reading than did those of less maturity, but they made less satisfactory progress than did those whose mental age was six months greater. The gain in ability up to six years and six months of mental age, however, was much greater than the subsequent gain.

6. A repetition of the experiment in 1929-30 with different teachers, different children, and different tests confirmed the earlier experiment in all its basic conclusions.

7. Consequently, it seems safe to state that, by postponing the teaching of reading until children reach a mental level of six and a half years, teachers can greatly decrease the chances of failure and discouragement and can correspondingly increase their efficiency.

Five—The Effectiveness of Drill in Handwriting to Remove Specific Illegibilities

HILDA LEHMAN &
LUELLA COLE

REPRINTED FROM THE ARTICLE IN *School and Society*, 27 (1928), 546-548, BY PERMISSION OF THE AUTHOR AND THE SOCIETY FOR THE ADVANCEMENT OF EDUCATION.

It is a common finding in studies of learning that errors are not eliminated unless they are recognized as errors. Mere practice without knowledge of what is correct and what is incorrect does not produce improvement. The present experiment indicates how effective teaching can be when it shows the child how to correct his specific errors, instead of merely saying "now let us practice our writing."

It is a curious circumstance that work on the handwriting problem to date has concerned itself almost entirely with the general appearance or comeliness of the writing, without reference to its major function, the ease with which it can be read. Pragmatically this last consideration is obviously the important one, and as a matter of actual observation it is clear that handwriting is often good looking and still baffling to the reader. Specific investigation of those forms which interfere with reading would thus seem desirable. And it would seem desirable also to determine whether drill in handwriting to eliminate illegibility might not produce more gain in legibility than the type of drill to obtain better form, which is now common. It is conceivable that such concentration upon specific malformations of letters might be very effective in improving general appearances also.

The experiment reported in this paper is based upon an investigation to determine specific factors interfering with legibility which has already appeared in print. This study shows that certain few specific malformations (for example, "d" written like "cl," "n" like "u," "r" like undotted "i," "h" like "li") account for the great majority of difficulties which readers find in reading handwriting samples. As a matter of fact the 27 most frequent specific illegibilities accounted for approximately 80 per cent of all "the holdups" in reading. On the basis of this investigation a chart was made exhibiting these most common errors and providing place for the tabulation of the illegibilities made by each child in a class.

The present experiment has to do with the efficacy of drill to increase the legibility of handwriting, based on the diagnosis of each child's illegibilities by means of this chart.

THE EXPERIMENT

The children worked with were 23 youngsters in a 4B class. The experimenter (Miss Lehman) first "diagnosed" the illegibilities of each pupil and then met the class one half-hour twice a week for nine and one-half weeks for remedial work. The gains made by this group were compared with a control group of 19 3A children who had the regular handwriting drill usual in the school. Both of these classes were under the same teacher and therefore (except for the special help given the 23 4B children) were subject to the same general instructional and classroom influences. The question then is as to the effects of the special procedure used by Miss Lehman as contrasted with the advances made by the 19 children in 3A during the same period.

At the beginning, the handwriting of both groups was appraised from handwriting samples obtained in two different ways. In the first place both groups wrote for two minutes the sentence, "The mail was late yesterday," and then the sentence, "Write a letter to him tomorrow." These samples were judged as to quality on the Ayres-Gettysburg scale, the median rating of five judges working independently being used as the final statement of quality on each sample, and the samples being so mixed that the judges did not know from which group a given sample came. The speed of writing, in terms of number of letters per minute, was also determined.

The children were next asked to write an original composition on an interesting theme. The handwriting of the compositions was also rated as to quality on the Ayres scale. Most important, however, was the measure of "readability." Each composition was read by three different readers, each reader timing himself with a stop-watch, to determine the total time taken. The figures were then made comparable by expressing legibility in terms of average number of letters read per second, on each composition. The two groups were essentially the same as regards handwriting, by both determinations.

Remedial work was then started with the experimental group. First the compositions were gone over, and all instances of the 27 illegibilities generally most common were tabulated on the diagnostic chart. The experimenter then pointed out to each child the illegibilities which he frequently made, and showed him how they could be corrected. She also adopted various practice procedures. Thus there was special practice for each child in writing those letters most commonly formed wrongly and lists of words containing these troublesome letters. Lists were also prepared illustrating to each child how his common illegibilities might cause confusion—such pairs as "dean" and clean" to show how failure to close the loop on a "d" might cause misreading. After this more formal work the children wrote dictation exercises and original compositions, the writing in all cases being gone over for illegibilities and made the basis for further remedial work. The program thus continued for the nine and one-half weeks' period—19 half-hour meetings in all. The same end tests were then given to both experimental and control groups; both groups wrote two different timed dictation exercises, and an original composition assigned without reference to handwriting. Table I shows the outcome.

TABLE I

MEDIAN STANDING OF A CLASS BEFORE AND AFTER 12 WEEKS OF DRILL IN LEGIBILITY, COMPARED WITH RESULTS FROM A CLASS NOT SO DRILLED

	Dictation Exercises				*Composition*			
	Speed		Quality		Quality		Legibility	
	Control	Exper.	Control	Exper.	Control	Exper.	Control	Exper.
Begin	46	51	31	32	30	32	17	19
End	58	69	34	46	30	41	14	29
Gain	12	18	3	14	0	9	–3	10

Speed is in terms of number of letters per minute, quality in terms of values on the Ayres-Gettysburg scale, legibility in number of letters read per second.

As will be seen from this table, the control group gained 12 letters per minute in speed of writing, while the experimental group gained 18; in quality the control group gained 3 points on the Ayres scale, whereas the experimental group gained 14. In other words, although in the work with the experimental group there was no emphasis on either speed or quality, distinct gains were made in speed and rather striking gains in quality also. In terms of the Ayres norms the gain is one grade in speed and one and one-half grades in quality, in the nine and one-half weeks' period.

On the compositions the gain in quality as rated by the Ayres scale was 9 points. And now as to the readability. The figures show the control group to have lost slightly in the number of letters of their writing which could be read per second (the loss is probably so small as to be of no significance). However, 10 more letters per second, of the handwriting of the experimental group, could be read at the end of the training than at the beginning. To put it another way, the readers were able to read the writing of the experimental group about 50 per cent faster at the end of the experiment than before.

THE GENERAL SIGNIFICANCE OF THE FINDINGS

The findings perhaps are so obvious as not to need comment. It deserves to be emphasized, however, that by direction of teaching effort straight at specific faults of legibility not only was legibility strikingly increased, but both speed and quality of handwriting were also very definitely increased. The educational moral seems obvious. The specific nature of educational problems and the need for a direct and specific attack upon these problems are points clearly instanced.

SUMMARY

1. The paper reports an experiment to determine the efficacy of drill in handwriting directed toward the removal of specific illigibilities in each child's handwriting.

2. The experimental group consisted of 23 4B children, and the control group of 19 3A children under the same teacher. Remedial drill to eliminate illegibilities was given the experimental group for half an hour twice a week for a total of 19 such lessons. Comparative gains were determined by handwriting samples obtained in dictation exercises at the beginning and end of the experiment from both groups, and evaluated in the usual way as to writing rate and quality on the Ayres-Gettysburg scale. Efficiency in writing was also determined by having both groups, at beginning and end, write an original composition, the legibility being measured by averaging the reading rates of three different readers.

3. It was found that in speed of writing, general quality or appearance *and* legibility as determined by rate at which the writing could be read, the experimental group made definitely greater gains than the control group.

4. It is concluded that drill aimed at specific illegibilities is much more efficacious than general drill in handwriting, in increasing not only legibility but also writing rate and general quality.

Six—An Experiment in Teaching History Backwards

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WILLIAM L. WALKER

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Here is another instance of the rewarding possibilities of research on learning in school situations. There is one weakness in this study, however. Other investigations have shown that to single out a group and treat it differently, designating it as an experimental group, will often cause increased group morale, heightened interest, and better performance almost regardless of what is done to the group. It is possible that the special attention given the experimental group rather than the method of teaching led to the improvement in learning. Can the student suggest how this criticism could have been obviated?

This article is the outgrowth of an investigation designed to test the merits of a plan of teaching history which has been proposed in certain quarters and which involves beginning with the past and coming down to the present time. The subject was American History in the junior high school.

The investigation was in the nature of an experiment in which one class was taught by the usual forward method and the other class by the backward method. Effort was made to equate the students as to all factors that would affect the outcome of the work, in order that any differences in results achieved might be rightly ascribed to differences in the effectiveness of the methods employed.

The total time for the experiment was 12 weeks and this time was divided into two periods of 6 weeks each. Group I used the backward method for the first six weeks while Group II used the forward method, and then the procedures were reversed for the second period, thus rotating out any differences that may have existed between the groups or between the difficulty of the material studied.

The same test was given to each class at the end of each period to measure immediate results, and an other test was given to both groups two months after the close of the experiment in order to measure retention or permanency of results. The tests were both of the new objective sort and also the essay sort. The essay papers were scored by another teacher who did not know about the experiment, in order to avoid the element of prejudice in marking papers.

The nature of the lesson materials for the experiment should be described more fully. During the first six weeks the topic was transportation; during the second six weeks it was communication. The backward method was employed in teaching transportation by having the students read the newspaper accounts of the effort made by Brock and Schlee of the United States Navy to break the world's endurance record in the air. In other words, the beginning on the topic of transportation was made in connection with the subject of aviation. This, of course, led back to Charles Lindbergh, who had participated in the air races at Los Angeles, and led further back to Darius Green and his flying machine and eventually back to the beginnings of the horseless carriage, the iron horse, the ox cart, the covered wagon, the steamboat, and Columbus's voyage of discovery in 1492. A similar approach was made in the second period of the experiment, which dealt with communication. The class began with television and the transmission of photographs, worked back to Marconi, Alexander Graham Bell, the laying of the trans-Atlantic cable, the invention of the telegraph, the origin of postal services, newspapers, and so on, reaching eventually back to Benjamin Franklin and his printing press.

The forward method covered the same material as the backward method, employed the same reference material, and sought to hold students responsible for knowledge of the same essential elements in the topics that were taught. The only difference was that the outline of topics was taken up in the order in which the events actually happened, beginning with Columbus or Benjamin Franklin and working down to the present time according to the chronological sequence of history.

RESULTS

Table I presents a condensed summary of results of the experiment, bringing out a comparison of the effectiveness of the methods in terms of immediate results at the end of the experiment and also in terms of retention results based on the test given two months after the close of the experiment. The important thing about the table is that the results are favorable to the backward method throughout and overwhelmingly so as far as the immediate results are concerned. The statement of chances, 480,000 to 1, indicates that if this experiment were repeated an infinite number of times under similar conditions in all particulars, there would continue to be an advantage in favor of the backward method of approach. The test for retention two months after the close of the experiment does not give nearly so great an advantage to the backward method and the chances are only 62 to 1 that further experimentation would continue to favor this approach.

TABLE I

RESULTS OF THE EXPERIMENTS COMPARING THE FORWARD AND BACKWARD METHODS OF TEACHING HISTORY

Test	*Method*	*Average for Class I*	*Average for Class II*	*Total*	*Difference Favoring Backward Method*	*Chances that the Difference is real* *
Immediate	Backward	38.18	35.53	73.71		
"	Forward	34.97	31.41	66.38	7.33	480,000 to 1
Retention	Backward	36.49	36.56	73.05		
"	Forward	35.66	34.38	70.04	3.01	62 to 1

* This column is derived statistically by comparing the difference with its standard error and referring to the necessary tables for the corresponding statement of chances. Space forbids including the detailed data for this process in this article.

It is natural for the difference to be less in case of the retention test than in the case of the immediate test in any such experiment as this because the results of the experiment itself are mixed with the influences of various other factors as time goes on, so that a child's achievement on the deferred test is not so directly indicative of the method of teaching, but is based on the amount and method of his reviewing as well as upon the other elements of learning that may have taken place due to his advanced study in the intervening period.

Naturally we should hesitate to generalize dogmatically on the basis of a single experiment, because much will depend upon the way the teacher works out the lessons, either by the forward or backward method, and because some topics or phases of history might not be so well approached by one method as by the other. The results are, however, sufficiently in favor of the backward approach to warrant a careful consideration of this method of teaching history. They should encourage further experimentation on the part of other teachers and in connection with other aspects of history work besides the two topics that were included in this experiment.

Seven—Getting Ideas from the Movies

P. W. HOLADAY &
G. D. STODDARD

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Movies, both recreational and educational, can readily be introduced into school programs. Is such use of movies actually educational? Do children learn incidentally when their interest lies primarily in the specific events of the movie drama? The study that is presented in abbreviated form below gives conclusive answers to these questions.

This investigation is an attempt to measure two effects of motion pictures on the memories of children: the retention of film content and the changes in quantity and accuracy of general information, including the direction and duration of the changes. What scenes in a picture stay best in a child's memory? To what degree are geography, history, and other information subjects incidentally taught by pictures created primarily for entertainment? These effects were studied by testing children on the content of selected motion pictures to which they had been sent.

PROCEDURE

Preliminary Steps in the Investigation

The first steps consisted in the development of techniques for picture analysis and test construction. Preliminary analyses were made of 17 pictures. These were written in story form, but the principal factor in each analysis was the determination of the testing possibilities of the picture. Analyses of pictures were made on two other occasions during the study. These picture analyses served three purposes: They showed whether or not the pictures were adapted to the conditions and purposes of the study; they were useful for practice in preparing questions about the different types of action shown; and they aided in the general classification of pictures according to types (emotional humorous, and the like) and according to use in testing.

As a further preliminary step in the experiment, two pictures, *The Gaucho* and *The Baby Cyclone,* were selected for experimental analysis and test construction. Each picture was analyzed by a number of persons who were expert in dramatics, in art and architecture, or in domestic science, as well as by a stenographer and by one of the investigators. Each observed items which fell within his field. The dramatic critic wrote characterizations of the principal actors, in which he mentioned as many illustrations from the picture as possible. The art teacher and the domestic science expert gave descriptions of clothing, furnishings, and modes of transportation. The stenographer wrote a brief summary of the plot and copied the titles and subheadings. The investigator listed items of historical, geographical, scientfic, or general interest which might have been missed by the others.

From the material produced by these analyses, tests of the type used in the first three pictures were carried far enough to insure the value of the technique employed. These tests were not completed because they were not to be used; they were for practice only. They showed, however, that it was possible to construct tests which would measure adequately the main factual items of a picture. This plan for the construction of tests was continued with minor variations throughout the experiment. In September, 1929, another picture, *The Dance of Life,* was similarly analyzed, and a few trial questions formulated.

Tests for the First Picture

The observation of the first picture used in the experiment, *Sorrell and Son,* was carried out as just described. A complete analysis of the picture was prepared as a basis for the construction of two tests, one of general and one of specific information. The general-information test consisted of 25 yes-no items and 10 multiple-choice items, covering the customs, history, and life in England during the period shown in the picture. All questions could be answered after seeing the picture if they were not known previously, but any of them could have been answered by a well-informed individual who had not seen the picture. Examples of these questions are:

Yes-no test

Are there large hotels anywhere except in America? (*Sorrell and Son*, General, Part, 1, No. 15.) The directions for this test ask the subject to write "yes" or "no" in front of each question according to the truth or falsity of the item.

Multiple-choice test

A famous English University is 1. Yale, 2. Harvard, 3. Princeton, 4. Oxford. (*Sorrell and Son*, General, Part 2, No. 3.) The directions ask the subject to underline the correct response. *

The percentages of correct responses on each item for each age group on each testing showed definitely that certain changes in mental content had taken place as a result of seeing the picture. The various testings for the general-information test of *Sorrell and Son* were:

Pretest: The general information test administered before seeing the picture.

General: The same test administered to the same groups the day after the picture.

One-Month General (1 General): The same test administered a month after the picture.

If item No. 15, Part 1, of the general-information test for *Sorrell and Son* is considered in terms of percentages of correct responses by each age group on each testing, the following data are obtained:

Age Group	*Percentages of Correct Responses* Pretest	 General	 1 General
Second–third grade	10	50	84
Fifth–sixth grade	48	70	76
Ninth–tenth grade	86	100	100
Adults	92	96	100

There are minor eccentricities in the data, but it is noticeable that each group gained in the average amount of general information concerning this item. The Pretest was given the morning of the day the picture was seen, and the General test was given the following morning. In this interval of one day the movie was the only known variation from everyday experience. Since a similar change was found in practically every correctly shown item used in the tests, the pictures can probably be held accountable for the changes in general information. Although the item described is of minor importance, the sum total of similar items covers an important sector of human knowledge. The percentages in such tables indicate the amount of general information gained from pictures as a whole since the pictures used in the experiment constituted an adequate cross-section of motion pictures.

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* A very much better technique was used in later testing, as illustrated in the following question:

A man who tried to conquer Russia and failed was (1) Charlemagne (2) Napoleon (3) Achilles (4) Wellington (5) Mussolini.

1( ) 2( ) 3( ) 4( ) 5( )

(*New Moon*, General, No. 17). The directions ask that a cross be placed in the parentheses with the same number as the correct answer. Scoring was done with a stencil laid over the test so that the correct and incorrect answers could be quickly detected and tabulated.

There is frequently a maturation effect shown in these tests. As in the case of the item quoted, the memory of the picture and the utilization of the general information gained from it are occasionally better over a longer period of time than over a shorter. In this case the percentages of correct responses were higher a month after the picture than they were the day after.

The 17 pictures used in the experiment were:

1928-30:

Sorrell and Son. Drama of wartime and postwar England. Silent.
Moran of the Marines. Humorous treatment of life in the marine corps at home and abroad. Silent.
The Midnight Taxi. Light drama of crook life. Talking sequences.
Kitty. Romantic drama of postwar England. Talking sequences.
Why Bring That Up. Humorous account of the rise to fame of the Two Black Crows of phonograph fame. All talking.
The Four Feathers. War in the Sudan and life in England. Very good shots of native and animal life. Silent.
Mysterious Island. Melodramatic fantasy of life in an undersea kingdom. Technicolor. Talking sequences.
Return of Sherlock Holmes. The mysterious adventures of the famous detective. All talking.
Show of Shows. A revue carried out in motion pictures. A number of scenes exploiting motion picture stars. Mostly technicolor. All talking.
General Crack. Romantic drama of war and love in Austria of 1740. All talking.

1930-31:

Tom Sawyer. The adventures of Mark Twain's boy hero and his friends.
Passion Flower. A drama of modern society, based upon the familiar triangle plot.
Gang Buster. A comedy of crooks and their actions.
New Moon. Musical romance of love and fighting at a frontier fort in prewar Russia.
Fighting Caravans. Stirring melodrama of life in a wagon train crossing the plains in the early 1860's.
Stolen Heaven. Drama of Palm Beach society life.
Rango. Native and animal life in Sumatra. This picture is quite scenic but contains a record rather than a story.

Equation of Groups of Observers

During the first year and a half the study was conducted at the University of Iowa. The three youngest groups of observers were taken largely from the elementary school and the high school conducted by the University. The adults consisted largely of graduate students in the University and their wives. Some students from the Training School of Iowa State Teachers College observed several pictures and answered the tests. Pretests of *Moran of the Marines* and *The Midnight Taxi* were also given at Washington, Iowa, and Cedar Rapids, Iowa.

During the last year the experiment was conducted at Ohio State University, Columbus, Ohio. This move was taken for three reasons: First, since Columbus is the capital of Ohio, the Ohio State Division of Film Censorship is located there, and previews of pictures could be obtained at the office of the State Division or at the local district offices of two motion picture concerns considerably in advance of the actual exhibition of the pictures. Second, it was possible at Columbus to obtain larger groups of observers who came from more varied types of communities than could be found in Iowa. Third, while obtaining the two first-named advantages, there was no loss in the administration of the study since the facilities of the Bureau of Educational Research of Ohio State University were made available for the use of the investigation.

The groups used in this portion of the experiment consisted of:

Pupils from Raschig Elementary School and Woodward High School, Cincinnati, Ohio—probably the poorest district in a large city. No Negroes were included either here or in any other of the groups.

Pupils from Avondale Elementary School and Walnut Hills High School, Cincinnati, Ohio—a high class residential district. The high school is distinctly a college preparatory school and draws superior students from all over the city.

Pupils from Heyl Avenue Elementary School and South High School, Columbus, Ohio—a middle-class district in a large city.

Pupils from the elementary schools and high school, Delaware, Ohio—a small town built around Ohio Wesleyan University.

Pupils from the elementary schools and high school, Wellsburg, West Virginia—a small town in the coal and iron district. There is also a small amount of manufacturing.

Pupils from the elementary schools and high school, Jackson, Mississippi—a town of 50,000 located in a district which is largely agricultural.

Superior adults, Columbus, Ohio—principally graduate students or young professors.

Thus a sampling of students from six types of communities with a wide range in home life and in occupational and financial status was obtained. The pictures were the same for all localities, and the mimeographed tests were administered under the same conditions. Variations in the test results should, to a large extent, represent variations in communities, and a combination of the data from all localities, comprising the tests taken by a total of over 2,000 elementary and high school students, should provide an adequate sampling and should represent approximately the findings which would be obtained by a similar study in any group of American communities.

For the first picture of the experiment, *Sorrell and Sons*, all observers for the picture were given all the tests, but this proved to be a poor arrangement. It is difficult to administer a test to a child satisfactorily more than once. In particular, the Pretest should not be administered to students who are later to answer the General or the 1 General. After taking the examination an observer goes to the motion picture with a changed attitude. As certain sections of the play unfold themselves, he recognizes questions he was asked previously and remembers the answers. These high points stay in his memory to a far larger degree than would be the case if he had attended the play with the sole motive and interest of personal enjoyment. For similar reasons, a test cannot be repeated for an observer after the picture, even though the two tests are a month apart, since the asking of a question based upon a portion of a picture raises that scene to a higher level of recall than the scenes which are unmentioned. At the end of a month it is doubtful whether the scene will have faded much, if at all, as the results of *Sorrell and Son* showed, and questions regarding it may even be answered with a higher degree of accuracy than immediately following the picture.

GENERAL RESULTS

1. The general information of children and adults is increased to a considerable extent by correctly shown information from motion pictures. On the tests of general information used in this study, the average scores of the groups from the second and third grades, the fifth and sixth grades, the ninth and tenth grades, and the superior adults increased 11.9 per cent, 30.7 per cent, 33.9 per cent, and 28.2 per cent, respectively. This retention is lasting, the percentages of increase between the Pretest and the same test given a month and a half after the exhibition being nearly as large as those obtained the day after the picture.

2. General information presented incorrectly by the pictures is frequently accepted as valid unless the incongruity is quite apparent. For the four age groups, decreases in average scores on the general information tests amounted to 7.7 per cent, 19.1 per cent, 33.8 per cent, and 37.5 per cent, respectively, when information was shown incorrectly. These decreases were relatively lasting, even increasing for the two youngest age groups and decreasing somewhat for the two older age groups. The content of a picture is accepted as authentic by a large percentage of the audience unless the errors contained are glaring.

3. Retention of the specific incidents of motion pictures is high. Children, even very young ones, can retain specific memories of a picture with a high degree of accuracy and completeness. The second-third grade group retained on the average nearly 60 per cent as much as the group of superior adults. This retention of scenes from motion pictures is high over a long period of time. A third of each age group was not tested for three months after each picture, yet the average scores for these groups were 91.6 per cent, 85.3 per cent, 80.8 per cent, and 83.1 per cent as high, respectively, for the second and third grades, the fifth and sixth grades, the ninth and tenth grades, and the adults as they were for equated groups at each age level the day after the picture. On many individual items the average percentage of correct responses of a younger age group was higher than that of one or more older age groups.

4. On some individual test items and occasionally on entire tests, an age group had a higher average retention on tests a month and a half or three months after the picture than it did the day after the picture. This situation occurred most frequently in the second and third grades, but it was common with all three of the older groups. Although a picture may seen to fade from consciousness quite rapidly, when the proper stimulus (the test) is used, the retention is remarkably high, and because of a settling out of unimportant details it may be remembered even better than at first.

5. Action was remembered best when it concerned activities such as sports, general action, crime, and fighting; when it had a high emotional appeal; and when it occurred in a familiar type of surrounding, such as home, school, or tenement. Action was understood least when it concerned unfamiliar activities such as bootlegging and business; when it had practically no emotional elements; and when it occurred in surroundings of an unfamiliar and interesting type, such as café and frontier.

6. Portions of the picture which were concerned directly with the plot of the story were remembered better by all age groups the

day after the picture than were items concerned with incidental details such as clothes, background, and casual speeches. This superiority increased over the period of a month and a half, and it increased still further on the tests three months after the exhibitions. The important items were remembered better initially; this superiority increased with the passage of time.

7. There was little or no difference in the retention of boys and girls in this study.

8. These high retentions occur with children in a detention home as well as with normal children.

9. The percentages of retention found by this study surpass to a large degree the percentages previously obtained from learning experiments. This is true in spite of the fact that in this experiment the incentive to learn was absent; the material to be learned was not even identified amid the mass of confusion items; and there was but one exhibition of each picture which occurred in a noisy theater filled with friends of the observers. Each of these points is in direct contrast to the elaborate procedures in use in other experiments in learning.

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